The British Journal for the Philosophy of Science

VOLUME IV FEBRUARY, 1954 NO. 16

IS PHYSICAL COSMOLOGY A SCIENCE?

A Discussion

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WHITROW.[1] Among the oldest literary relics that have come down to us are the cosmological myths of the ancient civilisations of the great river valleys of the Middle East. These myths have one feature in common : in all of them the physical universe is pictured with its centre in the region in which the myths originated. Thus, in many of them we can trace the idea that the world was created out of water. This was not mere fantasy because in both Egypt and Mesopotamia the land which supported civilisation had, in fact, been won by draining the river swamps, and so the idea arose that the whole universe had resulted by some divine being causing dry land to appear. According to one of the Babylonian legends concerning the creator, Marduk, ' In the beginning all the lands were sea. . . . Marduk bound a rush mat upon the face of the waters, he made dirt and piled it beside the rush mat.'

The great step taken by Thales of Miletus in the sixth century B.C. was to rationalise this kind of myth. He left out the imaginary Marduk and, postulating that everything was once water, regarded all things as having been formed out of water by a natural process like the silting up of the Nile. Thales, in short, is the traditional father of physical cosmology.

During the twenty-five centuries which have elapsed since Thales, particularly during the last three or four, our knowledge of many of the processes at work in nature has grown enormously. Nevertheless, our understanding of the physical world as a whole is still full of dark places. Writing in the journal *Endeavour* in January 1950, W. H. McCrea suggested that ' In its present state, cosmological theory probably impresses the general theoretical physicist as a highly unsatisfactory subject '. Indeed, many physicists find it so baffling compared with other branches of physics that they pay as little attention to current cosmological theories as to the original theory of Thales.

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[1] Based on a review of *Cosmology* by H. Bondi (Cambridge University Press, 1952) broadcast in the Third Programme of the B.B.C. on 23rd September 1952.

Recently in his short book, *Cosmology*, H. Bondi has written : ' The aim of this book is to present cosmology as a branch of physics ', a branch in fact of science, and he emphasises that he does not regard the subject as a branch of philosophy. Although there is much in Bondi's book with which I agree—I can commend it as a stimulating introduction to physical cosmology—I want to take up the challenge implicit in his view of the nature of the subject. This challenge can be expressed in the question : ' Is physical cosmology a science ? '

Most people, I am sure, do not regard the cosmology of Thales as a science in the same sense as, say, modern atomic theory. If modern cosmology, however, is, or is at least on the road to becoming, a science, then its character presumably must differ significantly from that of Thales' theory. Before examining whether and in what way this may be so, let us first consider the more general question : ' What do we mean when we say that such-and-such a subject is a " science " ? '

To frame a definition which is neither too narrow nor too wide is a problem of perplexing difficulty, and I do not believe that it has yet proved possible to formulate a complete definition. One feature which distinguishes science from other intellectual pursuits is the absence of questions of personal opinion and taste, such as occur in philosophy, history, literature and the arts. Although it would be too restrictive to say, as has often been said, that ' science is measurement ', nevertheless the crucial role that measurement plays in the advance of science is primarily due to the fact that, broadly speaking, the results of measurement are indisputable. There is general unanimity among men of science concerning the greater part of scientific knowledge and a general tendency towards universal acceptance.

This does not mean that there are no controversies in science. Far from it. As Professor Polanyi has said, ' Science does not advance like a steam roller '. The acquisition of new knowledge, the opening up of new perspectives is often a tortuous and painful process attended by much argument and counter-argument. This is due partly to the conservatism of human nature, partly to the fact that in most advances it is difficult at first to see the ultimate objective. The history of science time and again reveals two contrasting features : on the one hand the amazingly penetrating insight of the great pioneers ; on the other the periods of fumbling preceding the general clarification of many questions on which scientists are now generally agreed. The early history of science, particularly in the seventeenth century, was characterised by violent polemics. Even as recently as the latter part of the nineteenth century the professor of natural philosophy at Harvard used to tell his pupils that ' people now accept the undulatory theory of light because all those who formerly accepted the corpuscular theory are dead ' ! In our own day we have seen how the theory of relativity gave rise to prolonged controversy, and in turn Einstein himself has found it impossible to accept the new dynamics of the atom and Heisenberg's indeterminacy principle. Nevertheless, such controversies as occur in modern science tend, in the main, to arise at the periphery of knowledge. They may be regarded as the inevitable concomitant of any advance into the unknown.

Turning to physical cosmology, we are confronted by a very different situation. In this subject current differences of opinion run very deep, far deeper than in the other sciences. It is this obvious rift that leads most physicists to regard the situation in cosmology as highly unsatisfactory. At the same time it heightens the interest and appeal of the problems at issue. For mankind in general the valuable part of science is that which is finally settled and established. Statics, for example, would be a more interesting subject if its principles were still in dispute, but it would make crossing a bridge a much more hazardous procedure ! The question at issue, however, is not whether physical cosmology is interesting but whether it is a science, or at least on the road to becoming a science, similar in character to the recognised sciences. So far, it must be admitted, there are no signs in cosmology of that approach to ' universal agreement ' which is an essential hallmark of a science.

Bondi is fully conscious of the present lack of unanimity among cosmologists, and in his book he has given a graphic account of current rival theories. Indeed, he tells us that it seemed wrong to him to omit any of the major theories despite the fact that he himself favours one in particular. These different world-models present many fascinating features, but I must confine myself to two aspects only : one in which they differ and one in which they all concur. The theories of Lemaître, Eddington and Milne, widely as they diverge in many ways, all assume that the underlying ' substance ' of the world is something physically primitive, and that the history of the universe is the record of the changes in the form or pattern of this matter or substance. The problem of its creation lies outside scientific investigation because this substance is itself the first datum of science. Other theories, and in particular that of ' continuous creation ', which Bondi himself advocates, presuppose that the underlying form or pattern of the world is primitive, and that the history of the universe is the record of how new matter continually emerges and disappears so as to conserve the same world pattern. One can also imagine theories in which the substance and over-all pattern of the universe are either both conserved or alternatively both vary. Indeed, prior to the astronomical discovery of the red-shifts in the spectra of the great nebulae about twenty-five years ago, the main cosmological theories all assumed that both world-substance and world-pattern were conserved. The purest form of such a theory is that of Parmenides, who regarded the universe as an unchanging homogeneous sphere. But the opposite idea, that the universe is in a state of complete flux, goes back at least to Heraclitus —' we never step twice into the same stream '. The idea of Heraclitus is anarchistic and provides no firm foundation for the construction of a cosmological discipline. Nevertheless, it might be objected that this is no reason for assuming that the universe is not of Heraclitean type.

According to this classification there are four possibilities : either the fundamental substance of the universe and its general pattern are both conserved ; or they are both in a state of flux ; or else the substance is conserved and the pattern varies ; or vice versa. It is generally assumed, at least implicitly, that the physical universe must definitely fall under one of these types to the exclusion of the other three, but as to which this should be opinions differ. Indeed, it is often stated even more categorically that the universe is ' unique ', and that in consequence a satisfactory cosmological theory should, in principle, isolate a single world-model. Bondi himself stresses the alleged ' uniqueness ' of the universe, and at different times the claim has actually been made on behalf both of Milne's theory and also of the theory of continuous creation, that each has the merit of yielding a unique world-system.

This point of view, to which I once subscribed, I now regard as fallacious. The idea of the physical universe is a selective concept. We are ourselves within the universe. We cannot get outside it, not even in thought, but can only view it from inside from different mental perspectives. Moreover, the idea of the physical universe is not something which we merely find or come across in our experience of nature ; it is something which we read into it, as with those picture puzzles which represent things that we cannot distinguish at the first glance. When we have ' discovered ' the picture in them we have seen nothing new. The contour of this little mountain is now the mane of a lion and the knot in this tree-trunk is its eye. We had first read in this network of lines a certain structure, the landscape ; and now we have just read a second structure, the lion. This analogy, although used by Jean Nicod to illustrate a different problem, may help to clarify the point I wish to make. I believe that in the study of the physical universe as a whole, although one mode of description may automatically preclude the simultaneous use of others, no single mode alone will prove sufficient to elucidate all the phenomena.

Despite the many points of deep divergence between the various cosmological theories discussed in Bondi's book, there is, nevertheless, one significant point of agreement. They are all based on a common postulate which is often called the ' cosmological principle '. Bondi defines this as the postulate that, broadly speaking, ' the universe presents the same aspect from every point, except for local irregularities '. He goes on to comment : ' Although there are wide divergencies of view as to the significance, the necessity and the logical position of this postulate, the agreement as to its validity is very remarkable, and its utility is beyond doubt '. Bondi discusses the validity of this postulate in some detail. He begins with the assumption that experiments, if repeated at different times and different places under the same conditions, will yield the same results. This assumption has usually been discussed by natural philosophers as ' the principle of causality '. In a famous paper written in 1907, Philip Frank, inspired by the writings of Henri Poincaré, made the original suggestion that the law of causality is a convention. How do we know when an experiment has been repeated ' under the same conditions ' ? Frank argued that there is no exact method except to find out whether it yields the same result. Hence he concluded that the law of causality is not a statement about observable physical facts but is a definition of the expression ' under the same conditions '. Even if, like Einstein, we regard this point of view as incomplete, nevertheless, the principle of causality is primarily a rule of scientific method, and as such has no necessary connection with the cosmological principle, which concerns the distribution of matter in the universe. Bondi's idea that these two principles are intimately related is itself really a further principle, which, although of great theoretical interest, is open to question. A sceptic might argue that, in arranging the distribution of matter throughout the universe, the Creator was not greatly concerned with making life easy for the inquisitive physicist!

By his close identification of the principle of causality with the cosmological principle, Bondi is led to assume that the universe as a whole is in a steady state, so that it not only exhibits the same general aspect from any place but also at any time. The less restrictive form of the cosmological principle, however, which is the one uniting most current theories, allows for the possibility of variation in time. Bondi argues that this is a generalisation of what he calls the Copernican principle, namely, that the Earth is not in a central, specially favoured position in the cosmos. ' This principle ', he claims, ' has become accepted by all men of science, and it is only a small step from this principle to the statement that the Earth is in a *typical* position.' When reading this my mind went back to a paragraph near the end of Newton's *Opticks* where he speculates on this problem. ' It may also be allow'd that God is able to create Particles of Matter of several sizes, and in several proportions to Space, and perhaps of different Densities and Forces, and thereby to vary the Laws of Nature, and make Worlds of several sorts in several parts of the Universe. At least, I see nothing of contradiction in all this.'

Whatever we may think about the ' validity ' of the cosmological principle, there is no doubt of its utility. Indeed, it is one of the oldest principles in the subject. More than three hundred years ago Sir William Lower wrote to Thomas Hariot : ' We . . . were a consideringe of Kepler's reasons by which he indeauors to ouerthrow Nolanus' and Gilbert's opinions concerninge the immensitie of the spheere of the starres and that opinion particularlie of Nolanus by which he affirmed that the eye being placed in anie part of the universe, the apparence would still be all one as unto us here.' More than a hundred and fifty years earlier, Cardinal Nicholas of Cusa in his *De Docta Ignorantia* had also suggested that ' wherever the observer is placed in the universe that will appear to him to be the centre '. It is true that the world-systems of Dante, Ptolemy and Aristotle were not constructed in accordance with this principle, but nevertheless, when we go back to the cosmologies of the Ionians, such as Thales, and the still older cosmological myths of the Egyptians, Babylonians and Sumerians, we find that they too are based on a very primitive form of the same principle : the principle that some process which dominates a particular region in fact governs the whole universe. The ultimate origin of the cosmological principle lies deep in human thought ; it manifests itself whenever men attempt to explain the remote and unknown in terms of the nearby and familiar.

Modern physical cosmology is based on an incomparably wider range of knowledge than the cosmology of Thales. Does it also differ from it in character ? All theoretical science presupposes certain philosophical postulates, but as physical science advances men have tried to relegate these postulates as far as possible to the ' background ' of nature. In physical cosmology, however, the subject studied is the ' background ' itself, and so cosmology necessarily differs in character from the special sciences. Moreover, as these sciences evolve, the main questions change although the answers, once established and generally accepted, remain as part of the fabric of human knowledge. In the history of physical cosmology, however, there is a closer parallel with philosophy : as knowledge grows the answers continually change, but the basic questions tend to remain much the same down the ages. Indeed, the cosmologies of today, although highly sophisticated, do not differ so widely in character from the naïve cosmology of Thales as we often like to imagine. Certainly, modern physical cosmology can be regarded as a science in so far as we train ourselves to study it in the spirit of scientific method and recognise our hypotheses for what they are. Nevertheless, it is in my view rooted equally in philosophy and in empirical knowledge, as were the most ancient cosmologies although at a far more primitive level. Physical cosmology has been, still is and, from its very nature, will, I believe, remain a border-line subject between the special sciences and philosophy.

Bondi. I am very glad the challenge expressed at the beginning of my book has been taken up by Dr Whitrow in such a spirited and erudite fashion. It may therefore be desirable that I should state the reasons that lead me to hold views on this point that differ so markedly from his.

Whitrow has discussed the history of cosmology in some detail and I do not differ with him on those points. Our difference seems to arise from the use of a different definition of science. To my mind the most characteristic feature of any science is that it confines its attention to the establishment of connections between existing results of experiment and observation, and to the forecasting of new ones. The chief claim that can be made for any scientific theory is that it fits the facts, and forecasts (or has forecast correctly) the results of new experiments; the chief method of disproof of any theory is that it disagrees with the facts. I have deliberately used the expression ' chief method ' and not ' only method ' for there is another way of disproving theories, namely, by showing them to be logically (that often means mathematically) inconsistent. For example, modern developments of quantum theory, undoubtedly a branch of physics, have been largely concerned with removing logical faults from a theory that agrees with the facts.

It will be seen, therefore, that in established sciences experimental methods of disproof go together with logical methods, though the former predominate. Whitrow, however, defines a science primarily by the absence of questions of personal opinion and taste, and states that there is general unanimity among men of science concerning the greater part of scientific knowledge. To take the second statement first, it seems to me to be the mark only of a *well advanced successful* science that there should be such unanimity. In recent sciences, such as virus research or psychology, unanimity is absent. What is essential is only that there should be unanimity about the means of deciding what is correct and what is incorrect, that the *yardstick of experiment* should be universally accepted. How much knowledge has in fact passed this test, how great the fraction of work that has thereby gained universal agreement, is only a measure of the progress that has been made in the field.

The question of the absence of personal opinion and taste is far more difficult to discuss. On the one hand I am sure that these feelings play a far greater rôle in scientific progress than is commonly realised, and on the other hand I think that a great deal of present day unanimity is due to the present day uniformity of thought. All of us prefer to assume that gravitation and dynamics explain the motion of the planets rather than that, as in Kepler's view chains of force emanating from the *animae motrices* of the sun are dragging them round. Both theories appear to deal with the facts, and our preference for Newton's theory is only a matter of taste, though this particular taste is almost universal now. This liking for simplicity, for rational connections, has become part of the universal background of men of science. Disagreements do, however, frequently arise on what constitutes the simplest connection. Einstein's rejection of the uncertainty principle, to which Whitrow has referred, is frankly and openly based on personal taste.

It appears that little guidance can be expected from this approach and that the chief criterion of a science should be taken to be the universal acceptance of the possibility of experimental and observational disproof. Conversely, the failure of this criterion can be seen to be common to non-scientific pursuits.

What now, is the position in cosmology ? It is true that at present there are a number of conflicting theories in the field, but this is only a sign of the early state of development of the subject. What is significant for our question is how the theories compete. Although the adherents of some theories are particularly concerned with pointing out the logical beauty and simplicity of their arguments, every advocate of any theory will always be found to stress especially the supposedly excellent agreement between the forecasts of his theory and the sparse observational results. The acceptance of the possibility of experimental and observational disproof of any theory is as universal and undisputed in cosmology as in any other science, and, though the possibility of logical disproof is not denied in cosmology, it is not denied in any other science either. By this test, the cardinal test of any science, modern cosmology must be regarded as a science.

Whitrow has raised several other questions of great interest, notably the question of the law of causality, but a necessarily lengthy discussion would be out of place under the title of this discussion. I do, however, wish to take this opportunity to correct an impression Whitrow appears to convey at one point concerning my views. I do not and never did think that matter was distributed in the universe so as to make ' life easy for the inquisitive physicist '. I do, however, hold very strongly that, until evidence to the contrary is forthcoming, we must, in the interest of scientific progress, assume the distribution to be the simplest that is compatible with the known facts. The demand for the simplest possible theory is common to all science and should be applied in this as in all other scientific work.

Whitrow. Mr Bondi has made a penetrating reply to some of my main points and I would like to amplify these a little in the light of his criticisms.

Bondi suggests that the difference between us seems to arise from our different definitions of science. I believe that the principal point at issue is our different conceptions of the nature of cosmology ; but, as I introduced the more general subject of the nature of scientific method into my discussion, I will deal with this question first.

Bondi argues that ' the chief criterion of a science should be taken to be the universal acceptance of the possibility of experimental and observational disproof', and to this he adds the possibility of logical disproof as well. I have no wish to quarrel with the use of this criterion, and I do not dispute the crucial rôle played in science by these various forms of disproof. Where we appear to differ is in our conceptions of the nature of this criterion. Bondi appears to regard it as final and absolute, whereas I look upon it as a method employed in that thoroughgoing pursuit of general unanimity which, in my view, characterises scientific work. (I may perhaps be allowed to add that, as I was not primarily discussing scientific method in general, I emphasised this characteristic only, but I do not believe that it is the whole story. For example, I said nothing about the rôles of questioning and theorising in scientific method.)

Bondi himself uses the words ' universal acceptance ' with regard to the means of deciding what is correct or incorrect and adds that the yardstick of experiment should be universally accepted. The position is, however, more complex than this. For example, it has long been known that experiments by Miller seem to disprove the fundamental conclusion drawn from the classic experiment of Michelson and Morley that there is no ' ether-drift '. To date, however, Miller's results have not been incorporated into the general body of physical science and yet so far as I know no one has strictly disproved his results.[1] Instead, they have been left on one side as not fitting into the generally accepted pattern of the relevant branch of science. The desire for unanimity, that is, the desire to convince not merely oneself but others as well, underlies the scientist's appeal to observation and experiment (as well as logic). Indeed, I would say that in science there are no facts ' pure and simple ', but only facts which are generally accepted as such by those studying the subject in question. The important role of disproof in science, which has been so cogently argued by K. R. Popper, is intimately related to the self-correcting tendency of science and this, in my view, is another aspect of the pursuit of unanimity. Another aspect is the cumulative tendency of science.

I agree with Bondi that ' feelings play a far greater rôle in scientific progress than is commonly realised '. Indeed, I made practically the same point early in my previous statement, but there are two comments I would like to make on this statement. First, it seems to me that

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[1] This example has been chosen without prejudice to recent suggestions due to Synge, Gardner, *et al.*

Bondi has not understood the precise import of my remarks, for example, when he comments ' our preference for Newton's theory is only a matter of taste '. I was stressing the absence of *personal* opinion and taste, without prejudice to the suggestion that our almost universal acceptance of, say, Newton's theory is itself an expression of *almost universal* taste. Moreover, I fully admit that personal taste often plays an important part in scientific discovery ; it is in scientific proof, that is, at the stage when discoveries pass into the generally accepted body of scientific knowledge, that personal taste is eliminated or becomes ' universal '.

Passing now to cosmology, I repeat that I believe it is here we find the core of our disagreement. I agree that virus research is a good example of a new science. A hundred years ago there could have been no such science. Many of the differences of opinion in this field may be regarded, as I suggested without mentioning this particular example, as ' the inevitable concomitants of any advance into the unknown '. On the other hand, differences of opinion in physical cosmology are not a temporary phenomenon characterising a new science. Physical cosmology is one of the oldest, if not the oldest, field of man's intellectual activity. It is true that much in this field which now claims our attention was discovered only recently, but our ideas of the world as a whole form a long chain stretching back to the dawn of history. At times there appears to have been greater unanimity than today, but viewed in the light of history we see that the development of these ideas has followed a very different pattern from that which characterises a typical science. Bondi reasserts his belief that cosmology is on much the same footing as the other sciences, although he now suggests by implication that at the present time it is best compared with such subjects as virus research and psychology. (In the Preface to his book he claimed that it should be regarded as *a branch of physics*.) Virus research and psychology are recent sciences, and Bondi suggests that the conflicting theories in cosmology are due to the subject's ' early state of development '. I maintain, on the contrary, that cosmology is an old subject and that it is, moreover, a subject *sui generis*, intimately related to the physical sciences but not on quite the same footing, a subject with peculiarities of its own which I suggest can be appropriately labelled by the adjective ' philosophical '. To take a specific instance, the old question of the age of the universe, whether finite or infinite, appears to me to be one which cannot be decided by any purely logical and/or empirical proof or disproof, although, I might add, this does not mean that it is nonsensical to consider it.

One last word—I did not wish to suggest that Bondi himself consciously developed his own world-model so as ' to make life easy for the inquisitive physicist '. I said that this is how a sceptic might characterise God's intimate association of the principle of the indefinite repeatability of experiments and the cosmological principle.

BONDI. Dr Whitrow's second contribution has greatly clarified his point of view. I feel that we are moving, if not towards agreement, at least towards agreement to disagree.

The main point that now emerges is, as Whitrow says, that our conceptions of cosmology are different. He, in his first contribution, started with the Babylonian legends, whereas in my book the oldest work on cosmology explicitly mentioned is that of Olbers, only a century and a quarter ago. Although philosophical speculations on the nature of the universe may well be one of man's oldest intellectual pursuits, the scientific subject of physical cosmology is very young indeed. This subject is primarily concerned with establishing relations between, and analysing, observable data. Already in Olbers' paper, the chief aim is the drawing of inferences from an observable fact, the darkness of the night sky. My book, which started this discussion, deals only with this subject of physical cosmology. Apart from two contributions (due to Newton and to Olbers) no part of the subject is more than sixty years old, and very little dates back before 1916. In this subject the experimental yardstick is supreme, and this is the subject I claim to be a science. In connection with Whitrow's analysis of the definition of a science, I might add that I consider universal acceptance of the possibility of experimental disproof of any claim an absolute test of what constitutes a science, but of course I agree that in any individual application differences of opinion as to the reliability and finality of any experiment may well arise.

My opinion that cosmology is to be regarded as a science remains therefore unchanged, but I agree that there is also a body of philosophical work which cannot be regarded as scientific, but which is also concerned with the structure of the universe. In the main, however, the day of this philosophical subject ended when the redshifts of the nebulae were discovered, just as, for example, a large part of the subject of the nature of time was transferred from philosophy to physics through the advent of relativity.

It seems to me, therefore, that the situation can best be summed up by saying that there is a scientific subject of cosmology (which is the one to which my book was devoted) but that, particularly historically, there is also a philosophical subject of cosmology.

There are one or two other points in Whitrow's second contribution that I should like to discuss briefly. The first one concerns Miller's experiments. These experiments have not been incorporated into the general body of physics because they directly contradict the hypothesis of absence of ether-drift, and the consequences of this hypothesis have been confirmed by numerous indirect experiments of a far higher degree of reliability and certainty than Miller's experiments. In other words, when an extremely difficult and uncertain experiment contradicts a hypothesis otherwise exceedingly well confirmed by experiments, the results of this uncertain experiment are disregarded.

Finally, while I understand Whitrow's point of view about the finite or infinite age of the universe, I feel rather more optimistic than he does about the possibility of empirical proof. Although a direct proof is of course as impossible as it is to see a neutron, the consequences of, say, an infinite age may be susceptible to proof or disproof just as the consequences of the postulated existence of neutrons are. Observations can decide that the universe behaves *as if* its age were infinite (or finite, as the case may be), just as experimental nuclear physics indicates that matter behaves *as if* nuclei contained neutrons.

THE RÔLE OF MODELS IN PHYSICS*

E. H. Hutten

I

Most scientists today are not very satisfied with the account of scientific method given by the philosophers. It is certainly true to say that the presentations found in books on the philosophy of science still show the traces of the Bacon-Mill tradition. But even where the misconceptions of epistemology have been left behind, there remains some basis for dissatisfaction : for the scientific method talked about by the philosophers seems strangely remote from what scientists believe to be the method they follow.

I must hasten to add that it is not necessary to assume that the scientists are always right in their criticism. The scientist can often do his work without an explicit appeal to general concepts and to logic ; training and skill guide him to his result. Experimentalists therefore tend to confuse the practical procedures employed in obtaining results with the theoretical methods designed to explain these results. But, I think, most will agree that all present methodologies leave a wide gap. To fill it we must keep as close as possible to the expressions actually used by scientists. We must take as an example a modern theory and discuss actual laws, instead of illustrating scientific method by means of old-fashioned and very simplified examples. Indeed, what is often exhibited as an instance of a scientific law, e.g. the sentence ' All swans are white ', can hardly be recognised by anyone as even remotely resembling such a law. Surely we never have exceptions to the law so conveniently at our disposal (like black swans) when we consider, for example, Newton's laws of motion. The use of such biased and over-simplified examples has seriously affected our understanding of scientific method. Similarly, the theories usually offered as specimens represent not even a small part of what, at least for physicists, is the simplest theory, i.e. Newtonian mechanics.

However, I must again come to the defence of the philosophers, at least to some extent. For scientific theories, even Newton's mechanics, are very complex ; and more modern theories are still more difficult to understand, since they treat of things which are far removed from ordinary experience. There is, then, some reason why we do not practice logical analysis on a modern theory, particularly when taken in its entirety.

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* Paper read to the Philosophy of Science Group, 8th June 1952.

If, however, we want to make progress, we must consider, in the first instance, the expressions exactly as scientists formulate them. This necessitates not only employing the conceptions of modern logic and semantics, but also developing them further. I want to talk here about a concept usually neglected by philosophers but always used by scientists, that is, the model. My aim is to try to show what is meant by ' model ' and what rôle models play in theory construction.

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When we ordinarily speak of models, we mean a *model of* something, that is, a construction of wood, wire, etc. The doll's house is a model of an actual house. The model of a crystal is a lattice work. The billiard ball may be taken, under certain conditions, as a model of an atom or molecule. The tetrahedron, under other conditions, is the model of the carbon atom.

To begin with, a model is thus a three-dimensional representation of an object. The model is usually larger or smaller than the object ; and we then speak of a scale-model in which the proportions, i.e. the structure of the object, are kept unchanged. The model is designed to show the relations that exist between the constituent parts of the object. On occasion, we are satisfied with a two-dimensional representation, or cross-section, of the object ; and so we arrive at the model as picture or diagram. Originally, however, the word ' model ' denoted an architect's plan. There are all sorts of models ; they vary from the crudest mechanical construction to the geometrical pattern. In this use, the model has mainly a *psychological* function ; it gives a visual representation of something which is not easily visualised, e.g. because of its size. This is also described as the *heuristic* and *pragmatic* use of the model.

But we may also mean a *model for* something—the model shows how a thing behaves. We do not believe that the carbon atom is actually a tetrahedron ; rather, the model shows how the atom can combine with others to form a compound, that is, how to understand the concept of valency. Over and above the mere representation, the model *explains* how something happens, i.e. a process, or a causal sequence of some sort. The electronic computer is a working model for certain brain processes. In this way the model has also a *logical* function; and it is for this reason that models are important in science.

Whenever we encounter a new situation, we try to orient ourselves by comparing it to a familiar situation and so to evaluate it. From this attitude arises the *normative* function of the model, or the model as ideal and paradigm, e.g. the model house. This function is outside the scientific domain, though it has created certain difficulties there. In science, we merely want to explain a new or unfamiliar phenomenon, and so we try to account for it in terms of what we already know, or to describe it in a language which is familiar to us. That is, the model is used to provide an *interpretation*. Thus, the motion of electrons in an atom is represented by the planetary model; again, we interpret an expression of quantum theory by expressions taken from classical mechanics and electricity. A sentence belonging to a more advanced theory is interpreted by a simpler theory and ultimately, if need be, by the concepts current in ordinary language.

At one time it was believed that we have a model only if we can actually construct it; but it was soon seen that though psychologically useful, such models were often logically very inadequate and even misleading. The mistake of modelling every physical process on a mechanical one was recognised with the advent of modern physics; just as today most of us object to a physical model for a psychological, or biological, process. Although some have claimed that, therefore, all models should be proscribed, it is of course only the use of the mechanical model for non-mechanical processes that has to be restricted or, possibly, given up. From this mistake we learned a more cautious use, and a better understanding, of models. We do not, and need not, always rely on models taken from mechanics; other theories as well may furnish a model.

There are, however, three general conditions. First, we start with a simple physical situation and, from it, take over mathematical symbols and other expressions in order to describe another situation. This is trivial: whenever we invent a new way of speaking, we must start—at least in science—before language just as we must terminate after language, i.e. with an experiment. But the condition will remind us that the model is used to build up a new terminology. Second, a copy correct in all details, or a photograph, is never spoken of as a model: there is always some element that is changed or left out in the model as compared to the thing of which it is a model. Models, therefore, can be misleading since we tend to identify them with the things they are models for. Third, the model must be familiar to us; we must know how to use and to describe it: otherwise it cannot serve as an aid in explaining unfamiliar experiences. For to interpret is to make clear the meaning of an obscure expression in an understandable language.

3

Let us take a specific example. In nearly all theories of physics we employ the model of an *oscillator*. The motion of a simple mass point acted upon by a force proportional to its displacement is easy to visualise. Moreover, the oscillator can be made more complicated by introducing friction and impressed forces, and so a variety of physical situations can be represented by it.

The applications of the oscillator model are too numerous to mention. For instance, consider the classical theory of the specific heat of solids, i.e. the Dulong-Petit law. We regard the atoms of the crystal lattice as linear oscillators so that there are $3N$ of them, if N is the total number of atoms present. Since the energy of the oscillator is, on the average, half kinetic and half potential, we assign—on the basis of equipartition—$\frac{1}{2}kT$ to each part of the energy; so we arrive at 6 cal/mole as the specific heat of a solid.

Consider now another, slightly more advanced, application, e.g. the propagation of light in a dispersive medium. Here we take the electron bound to its parent atom as the oscillator. We represent a medium polarised by an electro-magnetic wave passing through it as an assembly of such oscillators. We find then an expression for the refractive index as a function of the frequencies of both the oscillator and the light wave, i.e. the dispersion formula.

This application of the oscillator model can be pushed further. An electron vibrating under the influence of an external force is not only a mechanical oscillator, but also a Hertzian dipole. The oscillating dipole is the model for a source of radiation. So we can incorporate into dispersion theory the idea that the oscillating electron emits secondary wavelets which superpose with the incoming wave; and this allows us to describe the propagation of light in the medium in more detail.

Then, let us consider a still more unfamiliar situation. Take the cavity of a black body as an assembly of *virtual* oscillators—that is, the aether molecules are assumed to be oscillators, but of course there is no aether. The possible number of oscillators (or their modes of vibration) gives the amount of energy that may be emitted by the black body, i.e. the Rayleigh-Jeans law.

Finally, let us quantise the oscillator; the energy of vibration is divided into multiples of a smallest unit. This quantisation is most easily shown by a geometrical model, that is, by drawing the energy surface of an oscillator in two-dimensional phase space. And so we can go on.

What then, is our model? In the first instance, a simple process that is represented by the motion of a ball to which a spring is attached, the other end of the spring being fixed in space, like this:

This is a useful picture; but no one really believes that the many things and situations in which the model is used are fully described by it. What then, is the logical function of the model?

(1) The model allows us to set up a mathematical equation, i.e. the equation of motion. We can then derive other mathematical expressions, e.g. for the potential and kinetic energy, as in the theory of specific heat. We have now at our disposal a mathematical formalism so that we can symbolise the new situation in which the model is applied. It has been said that ' by a model we . . . mean any physical . . . system which has a similar relation-structure to that of the process it imitates '.[1] This realist language can be avoided by simply saying that the model gives the *syntactic* (including the mathematical) rules for using the expressions by which we describe the process. The model is the better the more completely it provides the syntax of the new theory.

(2) But to have the syntactic structure alone does not suffice; we need also to know to what the expressions refer or how to interpret the equations. The original usage of ' oscillator ', or the physical situation in which oscillators are originally and primarily used, sets the standard for describing a new situation and suggests a mechanism. The model introduces a descriptive *key concept* and indicates *semantic* rules. In the specific heat theory, the universe assumed consists only of atoms (or molecules) exhibiting a certain (thermal) behaviour; that is, the individuals we speak about have given properties, and so the universe of discourse is demarcated. We have now at our disposal a way of speaking, or a terminology. We know what the atom can,

[1] K. J. W. Craik, *The Nature of explanation,* Cambridge, 1952

and cannot, do in this situation since we can describe its possible behaviour in terms of the oscillator. The original interpretation of the mathematical equation is kept to some extent. The problem is to say clearly to what extent it is kept.

(3) For the import of the model lies in this fact that the new situation in which the model is applied is not exactly the same as the original situation, or as the standard which serves as model. The model is in some respects, even in the main respect sometimes, like the process or situation it is used to describe, or like the theory it interprets. But precisely because there is some significant difference between the theory and the model, we say that we have a description by model only, and not the full description. The new theory says more than the model. In other words, the semantic function of the model is to supply an interpretation. The interpretation, however, is only *partial*—otherwise it is not a model but the ' real thing '. Even if the model gives all the syntactic rules, it does not, and cannot, give all the semantic rules ; and an equation is always capable of many interpretations. After all, the model is used to enable us to introduce technical concepts and to construct a more advanced theory which does not directly relate to experiment. We are forced to employ models when, for one reason or another, we cannot give a direct and complete description in the language we normally use. Ordinarily, when words fail us, we have recourse to analogy and metaphor. The model functions are a more general kind of *metaphor*.

(4) What serves as model is, then, something simpler, and more familiar, than the thing, process, or situation for which it is a model. For this reason, so many of our models are taken from Newtonian mechanics, as the scientific theory with which we are best acquainted and which is most easily related to happenings in the laboratory. Notice also that, in the examples given, the oscillator model becomes more and more tenuous ; but the possibility of regarding the energy of a black body as due to standing waves of virtual oscillators is still based upon the mechanical oscillator. That is, the model is a *link* between theory and experiment.

(5) It is important to emphasise, I think, that the model connects theory with experiment. We may say that we explain, and possibly test, the theory *in terms of* a model ; but we do not regard either the theory or the test made (i.e. the experiment) as the model. The model is, then, not an application of the theory : rather, we apply the theory with its help. The *Gedanken* experiment, for instance, is not a model ;

we use it in order to illustrate a known theory in the simplest way possible, e.g. the uncertainty relations are illustrated by the γ-ray microscope. The phrase 'model experiment' (which occurs very rarely, however), is usually interpreted in the sense of 'paradigm', that is, as an application κατ' ἐξοχὴν of a known, though perhaps difficult, theory rather than as an interpretation for an incompletely understood theory.

4

The oscillator may be called an *auxiliary* model, for it is used within the context of a wider theory. A certain, though limited, number of such models can be found in every theory of physics; and they may be employed side by side. The models are introduced into the theory at some crucial points; and there is some danger of confusing them, like mixing our metaphors. We speak of heat as an oscillatory motion of atoms in a solid and of the flow of heat through a solid. But whatever model we employ, e.g. the heat flow, or the electric current, etc., its virtue lies in the fact that we can write down immediately the relevant equation. There are, however, no mathematical models in physics: the equation by itself is not the model. The wave equation is a model only because we know it to represent the spreading of a wave through space. It is the interpretation which is attached to the equation due to previous application that we need for describing our experiments.

But there is also a more general type of model, i.e. the *main* model which provides an interpretation for a whole theory. The planetary model is the basis for the Bohr theory. The classical concepts are presupposed, e.g. of orbit, etc., and with their aid we explain what we mean by 'energy level', 'radiative transition', and other quantum concepts. It is true that, today, the Bohr atom is no longer of primary import; but it would be rash to say that the planetary model has been abandoned (we still use it, for example, for bringing in the classical theory of the radiating electron which we need so that we can compute the intensity of spectral lines). Nowadays, we prefer the wave and the particle interpretation. Both of them are taken from classical physics; and they function as somewhat more abstract (i.e. less visualisable) models for explaining what is meant by 'electron'. For both interpretations are neither complete nor adequate; they are models only, and they suffer from a peculiar deficiency (i.e. of complementarity). For this reason we prefer the statistical interpretation which we accept as 'true' and not merely as a model, though there is doubt that, even today, quantum mechanics is fully developed and its interpretation complete.

Or, let me take an example from classical physics. In the standard presentation of thermodynamics we employ the model of the heat engine. This is, as Maxwell said, 'an engine entirely imaginary—one which it is impossible to construct but very easy to understand'. The whole theory of thermodynamics, that is, the first and second laws, is formulated—not always too happily—in terms of the heat engine and the Carnot cycle.

The main concepts of closed system and of equilibrium state are exemplified by the heat engine itself (plus reservoirs) and by the position of the piston. The motion of the piston represents the reversible process, and two of the state variables, i.e. pressure and volume, are specified in this manner. Through the idea of efficiency of the engine the concepts of absolute temperature and of entropy can be introduced. Finally, we can draw a simple picture of the heat engine and we can represent the thermal process by a pV diagram of the engine cycle, etc. All this is found in the textbooks where, sometimes, we are told of the very serious limitations of the model.

It has been objected that the heat engine is an application of, rather than an interpretation for, thermodynamics.[1] I do not think that this can be maintained. The concepts of entropy and of absolute temperature are not immediately clear, and we need the engine in order to apply them. In fact, we force all thermal processes into the schema of the engine cycle, even those which can hardly be said to fit it, like radiation (e.g. to derive Stefan's and Wien's laws). This is exactly the reason why we regard the engine model as artificial and restrictive; the process-language based upon it is not very adequate; and we prefer Carathéodory's formulation. We apply a theory in an experiment which tests the theory; but the heat engine has never been taken as a test for thermodynamics.

The point is, then, that the model serves here for interpreting a whole theory. A complete terminology, that is, a technical or artificial language is introduced that allows us to formulate hypotheses and laws, e.g. Maxwell's relations. And the model links thermodynamical to mechanical concepts, like work and energy.

In this instance, we speak openly of the model of the heat engine. This is not always so. Usually the main, or basic, model is tacitly presupposed or, at best, vaguely described in the accompanying text. In Newtonian mechanics, for instance, we assume a universe consisting of particles between which central forces are acting. This is stated by Newton himself; he took the picture of moving particles for granted, saying '. . . that Nature may be lasting, the Changes of corporeal Things are to be placed only in the various Separations and new Associations and Motions of these permanent Particles '.[1]

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[1] By Professor Dingle, in the discussion which followed the reading of this paper.

It is, I hope, not a misdescription to take as a model the general picture of the world presupposed by Newtonian theory. We are given here a key concept, like particle, and we are also shown how to use it; for we can specify what situations and events are possible in this mechanical universe. We have thus a universe of discourse, and with it a rudimentary and therefore partial interpretation of the formulae of mechanics in a simple language which is close to the thing-language of every-day life. That is, all the main characteristics of a model are present here.

We may remember that the Cartesian model of the world was not very successful. The concepts of extension (i.e. the vortex) and of motion cannot take over the rôle that particle and force play in mechanics. One reason is that we do not know of any simple situation that may be described completely by Descartes' concepts and can serve as model. Galileo's ball rolling down the inclined plane can be described, however, in terms of 'particle' and 'force'; and these concepts are so exemplified in the situation from which we take the model.[2]

Similarly, in Maxwell's theory the model of the force field is presupposed. The universe is pictured as a region for which, at any point, we can indicate the direction and magnitude of the electric and magnetic force. Indeed, it was the simple picture of magnetic lines made visible by iron filings that suggested to Faraday the concept of field; and Maxwell then developed his 'mechanical conception' of the field by connecting electro-magnetic to ponderomotive (mechanical) force.

Though physicists do not always speak of a model when considering a whole theory, it is obviously not a serious extension of this term to describe the general frame of concepts underlying a theory in this manner. The advantage lies in that the model permits us to bring out the assumptions tacitly presupposed in a scientific theory ; for these assumptions are, at best, only vaguely mentioned in the context or perhaps indicated by a diagram, but often they are not even verbally formulated. Without being clear about these assumptions it is impossible, however, to understand the theory, that is, to know how to interpret the formulae.

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[1] Newton, *Opticks*

[2] I do not mean to imply that either Descartes or Galileo thought in terms of 'model', though, as a matter of historical fact, the word 'modèle' is used by Descartes (*Traité du Monde et de la Lumière*).

There are other reasons for employing the concept of model when explaining how we construct theories ; some of these reasons will be discussed later. At present, I merely want to point out the use scientists actually make of the term ' model ', both in order to interpret a part, and sometimes also the whole, of a theory.

5

It is generally held that the replacing of experience by experiment is a characteristic of science. The artificial experiment needs an artificial, or technical, language to describe it. Ordinarily, when we wish to speak about something new or unfamiliar, we employ a metaphor. A large part of our normal language is, after all, metaphorical. Starting from the infant's restricted vocabulary we build up a treasury of general and so-called abstract words. It is well to remember here that ' no word is metaphysical without its having first been physical '.[1] We are forced to extend the normal usage of our words and even to coin new words whenever we describe some new experience. The new words and usages follow, more or less, the established pattern of our language ; but they do not exactly coincide with it. It is this, sometimes hardly perceptible, difference that enables us to advance.

The model is very similar to the metaphor. With its help we build up higher theories from simpler ones, and make use of concepts that are specially invented and do not directly relate to experiment. But the model is more than a metaphor. While the metaphor suggests rules of usage, the model indicates more specifically some of these rules : the model *specifies* the meaning of an expression.[2]

Whenever we invent a new term, there is one thing we are told we must do : we must define it. However, definitions are of little interest, and hardly ever occur, in the formulation of a scientific theory. It is easy to see why this is so. For a definition, in the strictly logical sense, states the equality of meaning, or synonymity, of two expressions ; but what we need, to begin with, is to establish the meaning of an expression at all. This can be done only by showing the usage of the expression, i.e. by giving its syntactic and semantic rules.

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[1] J. H. Grindon, *Figurative Language : its origin and construction*, London, 1879

[2] A. Kaplan, ' Definition and specification of meaning ', *The Journal of Philosophy*, 1946, **43**, 281. The term is used here in a different sense, however.

Usually, we do not succeed in explicitly stating this meaning ; we can only suggest it by an example ; or we can specify it with the help of a model. In order to explain what models can do I propose a technical explanation, or explication, of what I wish to understand by ' specify '. The meaning of an expression is specified if some, though not all, of the (syntactic and semantic) rules can be made explicit (abbrev. ' indicated ').

The model indicates the rules for an expression in which a key-concept, or principal descriptive sign, occurs. For example, the oscillator model when applied in the theory of specific heat allows us to say this. The atom is a small mechanical particle ; it is bound to the crystal lattice with a force proportional to the displacement ; it has only kinetic and potential energy ; and so we know what the atom can do under the circumstances.

This is not all that can be said about atoms, even within this theory, e.g. their specific heat has not yet been mentioned. But it suffices to state that the universe contemplated here contains *N* individuals that are all alike and have one main property, i.e. energy. A rule of designation (holding for the class of individuals) would be given, then, by saying that ' atom ' designates a particle. Similarly, a rule of truth might be, e.g. ' The atom has such and such (kinetic and potential) energy ' is true if and only if the atom is an oscillator. Or, we can give a rule of range, that is, characterise a class of sentences or state-descriptions admitted by the model.[1] We describe the possible states in which the atom can exist in the crystal lattice, by simply referring to the equation of motion or, rather, to the equation for the energy derivable from it. We could say, e.g. ' The class of sentences admitted in the theory of specific heat is given by all expressions formed in accordance with the energy equation of the oscillator '.

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[1] R. Carnap, *Introduction to Semantics*, 1946. These rules, I suppose, would belong to what has been called ' descriptive semantics '. Like all linguistic rules, they are part not of the theory, but of the meta-theory.

Various formulations may be given to these rules ; and it may be that the logical terminology available at present is of little use for a language as complicated as the language of physics. The point I want to make is this. The model prescribes a context, or gives a universe of discourse ; it so sets a limit to what can be said ; the content of the theory, and the logical range of the sentences in it, is therefore determined ; and the meaning of the sentences is specified. The model provides, in this technical sense, a partial interpretation of a more advanced theory in terms of a simpler theory.

6

Whenever we want to explain something, there are always two tasks that confront us. First, we have to find suitable concepts or invent a way of speaking ; and, second, we have to propose specific hypotheses in terms of these concepts. The model supplies primarily a terminology ; it is introduced in order to allow laws and hypotheses to be formulated. This is certainly true of the main model, e.g. the heat engine of thermodynamics : if we accept it, then we can state Maxwell's relations, etc. It applies also to the auxiliary model, e.g. the oscillator ; for we take the model as support for a theory, for instance, the specific heat theory, or dispersion theory, or radiation theory, etc.

The model in physics brings with it an equation ; and an equation, or formula, is sometimes a law ; and laws are often confused with hypotheses : so the model is taken for a hypothesis. This happens also because a simple model, like the oscillator, is a partial interpretation for a part of a theory only and so gives little scope for directly formulating hypotheses. We cannot regard the sentence, e.g. ' An atom in a crystal lattice behaves like an oscillator ' as a hypothesis ; and no one has ever spoken of the oscillator hypothesis, or of the heat engine hypothesis, etc. The word ' hypothesis ' is rarely employed on this level of theory construction. If it does occur on a higher level, as in the phrase ' the hypothesis of central forces ' in mechanics, we are not prepared to subject the hypothesis to a test ; rather, it is a special assumption which is useful for formulating hypotheses. The distinction between universal, or higher level, hypotheses and more specific, or lower level, hypotheses has been made, of course, on occasion ; but even when made, it is quickly forgotten. If a hypothesis is to be testable, it is a very specific sentence, of limited range, and mainly concerned with what happens in the laboratory ; and so it is quite different from a law, and even more from a model, or from a theory. The model shows that much more belongs to the fixed conceptual frame of a theory than is usually acknowledged ; and that we have to restrict the range of our sentences if we want to assure their factual meaning and take them as hypotheses, that is, as probability-sentences or predictions.

In the classical theory of specific heat, for example, the equipartition of energy is expressed by a theorem or law though (certainly today) we know it to be untenable under most circumstances ; but the theory and the law stand and fall together. We *may* speak of a hypothesis when, applying the theory to the molecules of a gas, and assuming internal degrees of freedom, we want to predict the specific heat of a complicated molecule on the evidence of its structure. The hypotheses so formulated are very specific and, as it turns out, somewhat weak. It is the attitude of *probabilism* that created this confusion : everything we say in science is put forward tentatively, but this does not mean that every scientific sentence is a hypothesis.

It is, I think, vitally important to follow the actual usage scientists make of the terms ' theory ' and ' hypothesis ' ; and this holds also for the use of the term ' law '. We speak of the theory, not of the hypothesis, of dispersion. We may interchange, on occasion, ' theory ' and ' law ', e.g. in describing gravitation, though we never lose sight of the fact that these two terms have a different range : a theory usually contains many laws. But we always talk of Ohm's law, for instance, and we would not be understood if we were to speak of Ohm's hypothesis.

This may appear trivial and pedantic ; but behind it lies a different conception of scientific method. We may remember that Mach opposed what he called the atomic hypothesis since, so he said, everything could be explained by it. Indeed, such a hypothesis would be useless ; but we would like a powerful theory that can explain every physical phenomenon—it would represent the unified theory so many physicists long for. Mach did not see that atomism is a theory and that a theory proposes, first of all, a terminology or technical language ; and that theories are not confirmed as hypotheses are.

A model, then, may or may not be suitable ; it always has its limitations ; but it is not either true or false, or confirmable, or directly subject to what is usually called induction. The model functions in a different way.

7

A scientific theory is generally regarded as a hypothetical-deductive system. Euclid's geometry is accepted as a model—in the sense of 'ideal'—and so the logical order of the statements occurring in a theory is given by the hierarchy governed by the deducibility relation. From a minimum number of axioms we try to derive important theorems, or laws; and from these we then obtain hypotheses to be tested, and possibly to be confirmed, by experiment. This characterisation of a theory, however, is not sufficient; the analysis must be carried a step further. For a theory, at least of physics, consists of two parts: a formal calculus and an interpretation linking the theory to experiment. A formula has (extra-mathematical or physical) meaning only when interpreted.

But the model gives a partial interpretation only; it does not represent the theory itself. The oscillator is a model for the specific heat theory; this theory, however, contains more concepts and equations than those provided by the model, e.g. the concept of temperature and the equation for the specific heat. This brings to mind the way in which theories follow one another, that is, by successive approximations governed by the Correspondence Principle. Several years ago I discussed the rôle of this principle in theory construction and its relation to models; let me summarise what is relevant here.[1]

Classical mechanics, for example, is an approximation to quantum mechanics for the limiting case of high energies; similarly, relativity mechanics reduces to ordinary mechanics, for low velocities, etc. The correspondence principle requires that a new theory must reduce to the old one for the special case in which the refinement introduced by the new theory can be disregarded. It is a necessary (though not a sufficient) condition that the calculi of the two theories are in asymptotic agreement.

It is obvious that the approximation to, and the model for, a theory have something in common. Both are partial interpretations of a theory, though in a different sense. No physicist, I think, would regard a model as an approximation, at least not in the strict sense as given by the correspondence principle. We do not say that the oscillator theory (which is not a theory anyway) is a first approximation to the specific heat theory, or that the mechanics of the heat engine is an approximation to thermodynamics. The planetary system is a model for the atom, while mechanics as exemplified by the model is an approximation to quantum theory. The construction of higher theories by the correspondence principle and by the model are methods supplementing each other.

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[1] E. H. Hutten, 'Semantics and Physics', *Proc. Aristotelian Soc.*, London, 1949, **49**, 115-132

The partial interpretation of a theory by its approximation is unique ; the partial interpretation of a theory by its model is not. Indeed, the more models we have for a particular theory, e.g. of the atom, the more completely do we know it. The particle model of the atom as given by kinetic theory, the Bohr model, or the vector model, etc., each propose a conceptual frame in which it is easy to formulate, and give mathematical form to, specific hypotheses, say, about collisions or spectra. Each model provides a *partial* interpretation of atomic theory and represents, so to speak, a certain viewpoint ; and we have *multiple* interpretations of a higher theory in terms of a simpler theory. Similarly, we may have many auxiliary models within a single theory ; usually, they overlap and are mutually compatible though, on occasion, the models are alternatives. In this way we build up a more *complete* interpretation of a higher theory, that is of a theory which cannot be related directly to the laboratory language.

It is for this reason that we need the partial character of the interpretation by model. If the simpler theory could completely interpret the higher one, we should not have to invent the higher theory. The greater explanatory power, and the advance in knowledge, which the higher theory represents is made possible, and safeguarded, by the multiple but partial interpretations by simpler theories. The formalism of a theory may be interpreted, after all, in many different ways ; and so the meaning of a sentence within a theory (which is not formalised) is not easily specified and, still less, made completely explicit.

It has often been said that a theory is like a net that we spread in order to catch the phenomena. If this is a picture that can be accepted, then it is clear that the size of the mesh may vary and that the net may be mended with pieces of different size ; the net must also have a certain area, and so requires a good deal of material, before we can hope to catch anything. The net is held down only by a few anchors ; but this is enough to make a catch. In other words, our interpretations are made up of many different models ; there are not a few assumptions we must accept in order to hold the theory together ; though only a few sentences are needed to relate the whole theory to experiment.

The ideal theory we might want and an actual theory are not quite the same thing ; and it may be that the ideal is mistaken. Certainly, a physical theory as we know it today allows, and demands, more freedom of interpretation than is generally acknowledged.

8

If models and approximations afford merely a partial interpretation of a technical expression, how do we determine its meaning ? I want to sketch here, very briefly, how the model fits into the more general scheme of confirmation.

We construct higher theories and employ technical concepts when the expressions of ordinary language, or of the laboratory language, no longer suffice. They are needed just because we want to describe things and situations that are not encountered in everyday life, e.g. atoms. But all explanation must be based on experiment. Thus, there arises the *ladder* conception : we need a series of explanations that are linked together, so that we can touch bottom as well as reach the top. This is the picture usually presented ; but how are the steps of the ladder joined together ? How do we establish the meaning of a sentence belonging to an advanced theory, e.g. of quantum mechanics ?

Obviously, the ladder is shaky ; the steps are loose. It depends also on whether we go up or down the ladder. There are no purely logical steps (deductive or inductive) that lead from the lower to the higher theory, or from the model to the theory. To find the right model is a matter of imagination and skill. But once we have found it and constructed the higher theory, then we may go down the ladder in safety, following the correspondence principle. Therefore it is often said that the model may be abandoned after it has served its purpose. This would be so if our theories were given as formalised systems and if we could state explicitly all the rules necessary for determining the meaning of any sentence within the theory. In fact, this cannot be done for any present-day theory, and so we need the model also for climbing down the ladder to reach the experiment. We may know all the mathematics needed for quantum mechanics, but its interpretation cannot be said to be complete, in the strictly logical sense of this term ; and how about meson theory ?

But the approximation and the model are always inadequate ; they leave out just that factor by which a new theory differs from them and which represents its *raison d'être*. How, then, can we know the meaning of a sentence belonging to a so-called ‘ abstract ’ theory and referring to so-called ‘ fictitious ’ entities, like electrons ?

It is clear that this way of posing the problem queers the pitch. The terms ‘ abstract/concrete ’ and ‘ fictitious/real ’ have no place in science. It is equally clear that formal procedures, like deduction, reduction, or translation, cannot suffice for giving the (non-formal, extra-linguistic, physical) meaning of a technical expression. But this is no reason for rejecting any criterion of meaning as has recently become fashionable ; the theories of meaning tacitly presupposed are at fault. No physicist, I think, will ever stop asking about the meaning of a new hypothesis and demanding an answer in terms of an experiment.

It is, then, misleading to say that sentences of a higher theory possess a *surplus* meaning since they cannot be completely reduced to, or translated into, the sentence of a simpler theory or some observation sentences. This sounds as if a higher theory represented an illegitimate gain, the result of exploitation—while, in fact, this surplus represents an advance in our knowledge.

The first objection is that this suggests that we know only one, or basic, language in which all science must ultimately be expressed, e.g. an empiricist language or ordinary language, etc. However, languages, that is, various ways of speaking, may be either more or less technical ; but there is no ontological difference between them just as there is none between entities like chairs and electrons. We are quite capable of learning the language of quantum mechanics, and of finding out how it relates to experiment, just as we have learned ordinary language. Having learned it, we may also give the meaning of a technical expression, that is, if we have sufficient logical ingenuity for inventing rules of usage for these expressions. The empirical basis of science is not created by speaking in a certain way but by making experiments. We anchor higher theories to experiment through chains of interpretation, i.e. by model and by approximation.

The second objection is that the idea of surplus meaning raises the question of what this could possibly be. It suggests that we should have to accept as factual sentences that cannot be related to experiment ; and the factual content of a higher theory would become something very mysterious. An advanced theory certainly says more than a simpler theory which functions as approximation, or as model, for it. But this is because the logical range of the concepts employed becomes narrower. The concept of atom, for example, has a smaller range than that of particle ; it applies to fewer instances. Quantum conditions have to be imposed on the planetary model so that it can serve as basis for Bohr theory ; and so on. There is no surplus here, rather the opposite. So a factual sentence belonging to a higher theory does not contain an addition which is somehow left out when we test, or confirm, the sentence. When compared to the lower theory (or to the approximation and the model), a higher theory *adds* new conditions ; therefore the range of its sentences is *diminished* ; but the theory as a whole says *more*. ‘ We know more and more about less and less.’

The third objection is that the analysis of a technical expression is confused here with its inductive status. The meaning of any expression is given by the rules of the system to which it belongs. If we did not know its meaning, how could we ever test, or confirm, the sentence ? The criterion of meaning in terms of verifiability, or testability, merely states what sentence, within a theory, may be regarded as factual ; it does not state what the meaning is.[1] A factual sentence, at least in science, must somehow be related to facts ; and the approximation as well as the model make sure that this is so.

The correspondence principle and the model are conceptual tools, and their use by the physicist reveals a scientific method altogether different from what is usually assumed. Traditional epistemology has nowadays been superseded by the hypothetical-deductive method ; but, then, we must be more careful in stating what we mean by ‘ hypothesis ’ and ‘ deductive system ’. We must also investigate what rôle models, approximations, ‘ thought ’ experiments, etc., play in theory building ; this rôle may turn out, one day, to be only heuristic, that is, if and when we have succeeded in developing a better methodology. Today these concepts are indispensable. The current, rather idealised, notions of ‘ theory ’, ‘ law ’, and ‘ hypothesis ’ are not good enough for the job we want to do ; they are, however, basic since they determine what we mean by ‘ induction ’. If we want to avoid playing the game of induction in the traditional manner, we must follow more closely actual scientific usage.[2] Otherwise we shall not succeed in the technical task of giving a logical reconstruction of scientific method which is relevant to modern physics.

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[1] E. H. Hutten, ‘The Criterion of Meaning ’, a paper read to the Metalogical Society (London), December, 1951.

[2] S. Ceccato, ‘Il Teocono ’, *Methods*, 1949, 1, 34-54

WILLIAM WHEWELL'S PHILOSOPHY OF SCIENCE *[1]

A. W. Heathcote

William Whewell was born in 1794 and died in 1866. From the 1820s for the rest of his life he occupied an important place in British scholarship. The small group of friends consisting of Whewell, J. F. W. Herschel, Babbage, Peacock and de Morgan were largely instrumental in raising the standard of mathematical education at Cambridge in the earlier part of the century, so that ten years after Whewell's death Sidgwick can say that 'it is to Whewell more than to any other single man that the revival of Philosophy in Cambridge is to be attributed'.[2]

From second wrangler in 1816 he became a Fellow of his college, and assistant tutor. At the age of twenty-six he was made an F.R.S. From 1828 to 1832 he was Professor of Mineralogy at Cambridge, and in 1838 became Professor of Moral Theology. Four years later he was vice-Chancellor. Amongst his other activities he played a notable part in promoting the British Association, and was its secretary and president at various times, and also president of the Geological Society.

His writings started with treatises on mechanics and on dynamics, then entered the field of the history and philosophy of science, and finally passed after the middle of the century into works on moral theology. The first of his major works appeared in three octavo volumes in 1837—the *History of the Inductive Sciences.* It was a pioneer study and deserved the favour it received. In 1840 there followed *The Philosophy of the Inductive Sciences, Founded upon their History.* Later this was enlarged and divided into three parts, as the *History of Scientific Ideas* (2 vols., 1858), *Novum Organon Renovatum* (1858), and the *Philosophy of Discovery* (1860).

1840 was an unpropitious date, for the next year saw J. S. Mill's *System of Logic,* which won such popularity that it overshadowed Whewell's contributions, although Mill was indebted in fact to Whewell and to Herschel. Both of these deserved to be heard in their

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* Received 28. v. 53

[1] Based upon material used for a dissertation submitted for Part II of the M.Sc. examination in the History and Philosophy of Science at London University, 1952.

[2] H. Sidgwick, 'Philosophy in Cambridge', *Mind,* 1876, **1**, 241-242.

own right.[1] Whewell, for his part, says that he is attempting a systematic survey of the whole range of physical science and its history, so that insight may be gained into the essence and conditions of all real knowledge, and the best methods for the discovery of new truths discerned. He is not unmindful of his predecessors, many of whom set out on a similar venture in their own day. He devotes the *Philosophy of Discovery* to them, and to his contemporaries such as Ampère, Comte, Herschel and Mill. His own work stood apart by reason of its Kantian foundations, and it was unable to stem the current of empiricism, yet it was in many ways a notable contribution to the understanding of scientific method. It is this aspect of it with which this article is concerned.

I *The Antitheses of Philosophy*

The *History of Scientific Ideas* was the title given to the two volumes composing the first part of what later became his philosophical trilogy. The material was collected at the same time as that for the *History of the Inductive Sciences*, but in the former work we have the history of science in so far as it depends on Ideas. Our starting point is what Whewell calls the fundamental antitheses of philosophy. Some are quickly dismissed, such as Thought *v*. Things, Matter *v*. Form, and Reflection *v*. Sensation.

Whewell is more attracted by Ideas *v*. Sensations. In perceiving external things we connect the impressions of sense according to the relations of space, time, number, likeness, cause, and so forth. At least some of these kinds of connection can be contemplated distinct from the things to which they are applied ; and so contemplated Whewell calls them Ideas. The impressions upon the senses which they connect are sensations. The action of the mind is not, however, to create the object, though it may mould, combine and interpret it. At most, let us say, it half-creates the object.

> Without the relations of thought which we here term Ideas, the sensations are matter without form. Matter without form cannot exist: and in like manner sensations cannot become perceptions of objects, without some formative power of the mind. By the very act of being received as perceptions, they have a formative power exercised upon them, the operation of which might be expressed, by speaking of them,

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[1] See especially Part II of Herschel's *Preliminary Discourse on the Study of Natural Philosophy*, London, 1831.

> not as transformed, but simply as formed;—as invested with form, instead of being the mere formless material of perception.[1]

The antithesis Theories *v.* Facts is related to this. Facts are ' particular observations '; and since in observation the mind is active we can say that a fact is a ' combination of our Thoughts with Things in so complete agreement that we do not regard them as separate '.[2] While theories are built upon facts, facts themselves involve theories. Indeed, a true theory can become a fact. We are ' constantly transferring to the class of facts, opinions which were at first regarded as theories '.[3] With theories the Ideas are considered as distinct from the facts, while in the case of facts, though Ideas may be involved, they are not consciously separated from the sensations but so incorporated with the sensations that we ' do not see *them*, we see *through them* '.[4]

The situation is quite different with the remaining antithesis, that of Necessary *v.* Experiential truths. Here Whewell sees a clear-cut distinction.

> The separation of truths which are learnt by observation, and truths which can be seen to be true by a pure act of thought, is one of the first and most essential steps in our examination of the nature of truth, and the mode of its discovery.[5]

It is insisted upon, he says, by almost all the best modern as well as the ancient metaphysicians as of primary importance. The distinctiveness of his own treatment is that he widens the field of ' fundamental ideas ' in which this necessity operates.

2 *Necessary Truths and Fundamental Ideas*

From Kant, whose works he studied in his early days at Cambridge, Whewell derived the idea of the formative activity of the mind in perception, by which we cannot help but experience events in a certain manner, especially in relation to space, time and cause. Here there seemed to be a way of dealing with the problem presented by induction, the problem of how we can pass from a few instances which we have examined to affirm a general law. Whewell agrees with Hume that we are incapable of seeing necessary connection in any of the appearances of the world, but he concludes, as against Hume, that therefore we have some other source of knowledge than experience.

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[1] *History of Scientific Ideas* (henceforward *HSI*), vol. 1, p. 40 ; also pp. 34, 37, 38

[2] op. cit., p. 30

[3] op. cit., p. 49

[4] op. cit., p. 44

[5] op. cit., p. 60

The necessity and universality in our knowledge has its origin in the very structure of the mind itself. Necessary truths are those in which we perceive not only that a proposition is true but that it must be true. We can never distinctly conceive its contrary. The ultimate form of these necessary truths Whewell calls Fundamental Ideas. So far as space, time and cause go Whewell borrows his treatment of them from Kant; but then he goes in search of *a priori* principles underlying each of the several groups of sciences. The history of science he regards as the establishment and progressive clarification of these basic Ideas. For even necessary truths are only slowly seen to be such. The intuition of them is progressive.[1] Experience is needed to clarify our Ideas because, though not derived from experience, they can only be exercised upon experience.

The Fundamental Ideas form the foundation of the sciences by giving rise to 'inevitable convictions or intuitions, which may be expressed as Axioms',[2] when they are clearly and distinctly entertained in the mind. These axioms can be regarded as the Ideal portions of the various sciences. Because of the progressive nature of our intuition of fundamental ideas, a science at any given time will include much material which is not seen to depend upon these axioms and which therefore does not share their necessity. As we pass from mathematics through the physical sciences to botany and zoology, the ideal element has a decreasing share. The phenomena are less and less subject to any rules or principles which we can see to be necessary.

In the case of dynamics, Whewell argues that the laws of motion derive their axiomatic character from being interpretations of the axioms of causation, and the axioms in their turn exhibit the Idea of Cause under various aspects. Experience and observation were needed only to show how the axioms were to be understood, in what way, for instance, cause and effect, action and reaction were to be interpreted. The Laws

> borrow their *form* from the Idea of Causation, though their *matter* may be given by experience: and hence they possess a universality which experience cannot give. They are certainly and universally valid; and the only question for observation to decide is, how they are to be understood.[3]

For what he calls the 'secondary mechanical sciences'—sound, light and heat—he gives 'Outness of Objects and Media of Perception of

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[1] *Philosophy of Discovery* (*PD*), pp. 344, 477; *HSI*, vol. 1, p. 47

[2] *PD*, p. 336

[3] *HSI*, vol. 1, p. 267

Secondary Qualities' as the Fundamental Ideas.[1] In the 'mechanico-chemical' sciences we encounter Polarity as the Fundamental Idea —admittedly a late growth, refined out of such conceptions as the poles of a magnet, two opposite fluids, transverse vibrations and so on![2] For chemistry we appear to have the two Ideas of Substance and Affinity.[3] From the former he derives the axiom of the indestructibility of substance. He is cautious about the atomic theory, which is useful but not proved.[4] He is sure that the power of chemical combination is not mechanical, and he suggests an approach to the problems of affinity by the neglected route of crystallography. For the rest, we need only mention that the Fundamental Idea of morphology is Symmetry; of the classificatory sciences, Natural Affinity; of the biological sciences, Vital Powers.

Having reached this point we ought perhaps to remind ourselves where we started from, though Whewell scarcely appears to remember. We started with Ideas which express the 'element supplied by the mind itself, which must be combined with Sensation in order to produce knowledge'. Ideas, in his use of the term, are 'not Objects of Thought, but rather Laws of Thought'. They are 'Principles which give to our Notions whatever they contain of truth'.[5] In a critical study of Whewell's work we should have to ask why Whewell did not see the need for some sort of *a priori* deduction for his Fundamental Ideas equivalent to Kant's treatment of his Categories, and why he supposed them to be latent in us; and whether, since they are progressively intuited, they are able to certify the universality and necessity of our propositions in the way they were intended to do. Since our concern at the moment is only exposition we must leave such questions and turn to the more valuable contribution which Whewell made by his examination of the method of science.

3 *The First Stage in the Formation of Science*

(i) *Introduction*

In the *Novum Organon Renovatum*, which was an enlargement of the second part of the 1840 two-volume *Philosophy*, we have Whewell's detailed examination of scientific method. The title was intended to recall Bacon's *Novum Organum*, yet with this difference, that while Bacon endeavoured to give precepts and a method for the future tasks, we can do no more than hope that the analysis of what has been done may be instructive and may afford some guidance for the future.

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[1] *Novum Organon Renovatum* (*NOR*), Bk. I, Aphorism XVIII

[2] *HSI*, vol. 1, p. 369

[3] cf. *NOR*, Bk. I, Aph. XVIII with *HSI*, Bk. VI

[4] *NOR*, Bk. I, Aph. LXXIX; *HSI*, vol. 2, p. 49

[5] *HSI*, vol. 1, p. 34

The first of the four books into which the *Novum Organon Renovatum* is divided summarises in aphorisms the two preceding volumes of the *History of Scientific Ideas*. The fourth is devoted to scientific terminology, on which Whewell was an authority. Books II and III deal with method. But the stages into which the formation of science is analysed are given differently in different places,[1] and there is no simple way of equating them. We must regroup the material for ourselves as logically as we can.

The conditions for real knowledge, Whewell maintains, are that distinct and appropriate Ideas are exactly applied to clear and certain facts. The first stage in the construction of science is therefore one of clarification. Clarification of the ideas is what he terms the Explication of Conceptions. Clarification of the facts requires the Decomposition of Complex Facts into simpler components. These two produce the raw materials for science.

(ii) *Explication of Conceptions*

By explication Whewell means the clear development of conceptions from Fundamental Ideas, leading to their precise expression in the form of definitions or axioms whenever possible. The Fundamental Ideas are comprehensive forms of thought. The special modifications of these which are exemplified in particular facts are called Conceptions. Such Conceptions, he holds, involve in themselves certain necessary and universal relations derived from the Ideas. Only after labour and discussion have the relevant conceptions of any science been hammered out, and we must therefore never speak with contempt of ancient controversies. Controversy often issues in a definition, though explicit definition is not essential at each step, and in any case is the end rather than the starting point of an inquiry.[2]

(iii) *Decomposition of Facts*

Alongside the explication of conceptions in this first step towards science goes a clarification of the facts. We have acknowledged that fact and theory interpenetrate and are not sharply distinguishable, yet we must demand that the facts which are to be the material of science are taken only from real observations. Complex phenomena must be decomposed into elementary facts, 'clearly understood and surely ascertained'.[1] This will generally lead to the introduction of special terms such as hour, cycle, declination, reflected ray, which denote the simple facts, Whewell believes. At the same time we shall be connecting the decomposed facts by appropriate Ideas, distinctly and rigorously applied, thereby stepping within the precincts of science.

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[1] *NOR*, pp. 27, 141-143

[2] *NOR*, pp. 28-37; also pp. 183, 184

4 *The Second Stage: the Colligation of Facts*

(i) *Colligation and Induction*

The term colligation, 'a binding together', seems to have been introduced into logic by Whewell. He used it for the binding together of a number of isolated facts by a general notion or hypothesis. Colligation of Facts is a term which may be applied

> to every case in which, by an act of the intellect, we establish a precise connexion among the phenomena which are presented to our senses. The knowledge of such connexions, accumulated and systematized, is Science.[2]

Whewell may have preferred this term to Induction because induction had been used traditionally to cover several different processes. Yet he is not quite clear as to their relation. He can identify them, saying that 'Induction is a term applied to describe the process of a true Colligation of Facts by means of an exact and appropriate Conception',[3] or later say that induction has three stages: the Selection of the Idea, the Construction of the Conception, and the Determination of the Magnitudes.[4] He also speaks of these three stages as the Colligation of ascertained Facts into general Propositions.[5] Elsewhere he implies that the framing of an hypothesis is the Inductive act itself, for Induction bounds to the top of the stair at once

> by a leap which is out of the reach of method. . . . The Inductive Intellect makes an assertion which is subsequently justified by demonstration; and it shows its sagacity, its peculiar character, by enunciating the proposition when as yet the demonstration does not exist: but then it shows that it *is* sagacity, by also producing the demonstration.[6]

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[1] *NOR*, p. 57; also p. 51

[2] op. cit., p. 60

[3] op. cit., p. 70

[4] op. cit., p. 186

[5] op. cit., p. 187

[6] op. cit., pp. 114, 115

In these rather varying terms Whewell is at least trying to think of induction as it is employed in the construction of science. His main point is that it is more than a mere summary of facts, for a conception is superinduced upon the facts when an induction is made. He is also clearly aware of the importance of hypotheses.

(ii) *The Conception and its validation*

Facts bound together by Conceptions give rise to the general propositions of which science consists. Even the preliminary decomposition of the facts involves, however, some tentative conception. For the facts

> must be apprehended and analysed according to some Conceptions which, applied for this purpose, give distinct and definite results, such as can be steadily taken hold of and reasoned from.[1]

These conceptions derive from clear and distinct Ideas; but only by insight, by 'sagacity', can we find the Ideas which are appropriate.

To discover scientific truths, Whewell maintains, we make suppositions consisting either of new conceptions or of new combinations of old ones, until we find one which will bind the facts together. To some degree this is a process of conjecture. It calls for inventive ability, the power to devise imaginary schemes until at last the one is found 'which conforms to the true order of nature'.[2] In Kepler's work we can see 'lively Fancy playing with the Reason without interrupting her', since Kepler records his failures as well as his successes, and with most persons to try the wrong guess is the only way to hit upon the right one. Yet Whewell at once adds that the conjectures are not random, but 'are clearly conceived, and brought into rigid contact with facts'.[3] He strongly criticises Mill's treatment of hypotheses because Mill seems to think that the discovery of the laws and causes of phenomena is a

> loose haphazard sort of guessing, which gives 'plausible' explanations, accidental coincidences, casual 'harmonies', laws, 'in some measure analogous' to the true ones, suppositions 'tenable' for a time, (which) appears to me to be a misapprehension of the whole nature of science.[4]

A mind is needed at once quick and fertile in suggesting, scrupulous in comparing its hypotheses with the facts, and ready to abandon any not supported by the facts. Whewell stresses also the value of hypotheses which turn out to be false or imperfect, and that the valid hypothesis reveals itself by predicting phenomena yet unobserved which are of the same kind as those which it was invented to explain.[1]

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[1] *NOR*, p. 63

[2] op. cit., p 78

[3] op. cit., p. 79

[4] *PD*, p. 274

A curious feature of Whewell's account is that he does not equate Conception with Hypothesis. A Conception—of which he instances force, for co-ordinating mechanical phenomena, or concord for musical phenomena—is regarded as an unfolding of the relevant Fundamental Idea, and it is ' very often clothed in a hypothesis ', so that the hypothesis then serves ' at first to facilitate the introduction of a new Conception '.[2] We therefore ask, how do we arrive at a Conception ?

(iii) *Rules for the Construction of the Conception*

Whewell gives certain general rules for the construction of a Conception. When quantities are involved he mentions the particular methods of curves, means, least squares and residues. For induction depending on resemblance he gives the law of continuity, the method of gradation, and the method of natural classification.[3]

What he calls the method of curves is simply that of plotting a graph. The efficacy of the method depends upon the ability of the eye to detect a regular curve, which is to be drawn amongst rather than through the plotted points ; in this way casual errors of observation are corrected and the laws which the observed quantities follow may emerge. By the method of means Whewell thinks only of the arithmetical mean and its value in eliminating errors and irregularities. The method of least squares is a particular version of the method of means and rests upon the presupposition that small errors are more probable than large ones.

The method of residues means the detection of an unexplained residue in phenomena already covered by a known law. Herschel, to whom Whewell refers for illustrations, states the principle thus :

> Complicated phenomena, in which several causes concurring, opposing, or quite independent of each other, operate at once, so as to produce a compound effect, may be simplified by subducting the effect of all the known causes, as well as the nature of the case permits, either by deductive reasoning or by appeal to experience, and thus leaving, as

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[1] *NOR*, p. 86

[2] op. cit., p. 67

[3] op. cit., chap. vii

> it were, a residual phenomenon to be explained. It is by this process, in fact, that science, in its present advanced state, is chiefly promoted.[1]

The Law of Continuity is a method intermediate between the methods applicable to quantities and those depending on resemblance. It asserts that

> a quantity cannot pass from one amount to another by any change of conditions without passing through all intermediate magnitudes according to the intermediate conditions.[2]

Whewell adds that it is a test of truth rather than an instrument of discovery. It appears in a more positive form as the method of gradation. This consists

> in taking a number of stages of a property in question, intermediate between two extreme cases which appear to be different. This Method is employed to determine whether the extreme cases are really distinct or not.[3]

As instances he gives the breaking down of the rigid distinction between electrics and conductors by Faraday, and his proving that electrolytic decomposition is not due to attraction by the electrodes.

The final method is that of Natural Classification. The stress is upon ' natural ', for it is only the natural classification which makes general propositions possible. It means ' grouping together objects, not according to any selected properties, but according to their most important resemblances '.[4] An example he gives is the distinguishing of geological strata by the fossils they contain.

These methods in general lead us to ' laws of phenomena ', and so to the choice of a Conception or hypothesis. The colligation of facts by the appropriate conception is nevertheless more than simply an economic summary of the facts, Whewell affirms.

(iv) *The relation of the Conception to the Facts*

Whewell steadily insists that

> an Induction is not the mere sum of the Facts which are colligated. The Facts are not only brought together, but seen in a new point of view. A new mental Element is superinduced.

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[1] ' Preliminary discourse on the study of Natural Philosophy ', 1842 printing in Lardner's *Cabinet of Natural Philosophy*, p. 156.

[2] *NOR*, Aph. XLIX, p. 220

[3] *NOR*, p. 220

[4] op. cit., p. 228

This corrects the common view that induction is merely the collecting of a general proposition from a number of particular cases. A new element is added to the combination ' by the very act of thought by which they are combined '. It is because of this connection or Conception that we reach a new generality.

> In order to obtain our inference, we travel beyond the cases which we have before us; we consider them as mere exemplifications of some Ideal Case in which the relations are complete and intelligible. We take a Standard, and measure the facts by it ; and this Standard is constructed by us, not offered by Nature.[1]

The idea of momentum, for instance, is a creation of the mind, ' brought in among the facts, in order to convert their apparent confusion into order '.[2] The same is true, Whewell thought, of Kepler's first Law, but Mill disputed with him over this.[3] In general terms Whewell held that by induction we mean not merely that ' these particulars, and all known particulars of the same kind, are exactly included in the following general proposition ', as Mill held, but that

> these particulars, and all known particulars of the same kind, are exactly expressed by adopting the Conceptions and Statement of the following Proposition.[4]

In this he was surely right.

(v) *The Logic of Induction*

By the logic of induction Whewell means the presentation of an inductive argument, the facts and inference, in such a way that the evidence for the inference is clearly seen. The way he would do this—and he gives an example of it—is by drawing up Inductive Tables, which show the successive steps of induction, setting the facts at one side and the inference at the other. The division is not rigid. At any stage

> the Inductive proposition is a Theory with regard to the Facts which it includes, while it is to be looked upon as a Fact with respect to the higher generalisations in which it is included.[5]

The Tables do not reveal ' the formative act exerted by the understanding ' whereby the facts were colligated by a conception. Yet they conveniently display both the order of discovery and the process of verification of discoveries once made. Tracing the Tables in the reverse direction we follow the track of the deduction which subsequently confirmed each inductive inference.

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[1] *NOR*, pp. 71-73

[2] op. cit., p. 74

[3] Mill, *A System of Logic*, 3rd ed., London, 1951, pp. 291-314; *PD*, pp. 248-273

[4] *NOR*, pp. 110, 111

[5] op. cit., p. 116; also pp. 97, 105

(vi) *The Induction of Causes*

The inductive truths which we obtain are, Whewell holds, of two kinds, laws of phenomena and theories of causes. It is necessary to begin in every science with the laws of phenomena and perhaps for a long while to be content with going no further. But it is impossible in the long run that we should be satisfied to stop short of a theory of causes. This step requires great talents and great effort, for it involves the formation of distinctly new conceptions. It is like feeling in the dark for an object we cannot find. If we ask what kinds of causes are to be admitted it is hard to answer. Whewell refers to the law of gravitation as co-ordinating the laws of motion of astronomy, and to fluid properties invoked to explain the phenomena of sound. In chemistry the forces operating are certainly not gravitational, so that we must conceive new kinds of force to make chemical phenomena intelligible.[1] In short, he says that we can admit forces, mechanical, chemical, vital, for our causal explanations.

It will be remembered that Comte divides hypotheses into a small class which relate simply to the laws of phenomena, and a larger class which he dismisses as anti-scientific and chimerical, that 'aim at determining the general agents to which different kinds of natural effects may be referred'.[2] A permissible hypothesis must relate exclusively to the laws of phenomena and never to their mode of production, Comte dogmatically asserts. Whewell found little in Comte's contribution to the philosophy of science of which he could approve, and he particularly objected to this attempt to proscribe scientific inquiries in such a fashion. Indeed, it is not possible. Comte condemns both the emission and the wave theory of light. Very well, says Whewell, let him point out some other way of connecting double refraction with polarisation. How could polarisation of light have been reasoned about except by imagining transverse vibrations, or some equivalent supposition; or the flow of heat, except in terms of caloric or of moving particles?[3]

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[1] *NOR*, p. 121

[2] A. Comte, *The Positive Philosophy of Auguste Comte freely translated and condensed*, by H. Mertineau, London, Truebner, 2nd ed., 1875, vol. 1, pp. 186, 187

[3] *PD*, p. 230; *NOR*, pp. 125, 126

5 *Conclusion*

We have limited ourselves to a brief exposition of Whewell's philosophy of science, especially in so far as it is concerned with scientific method. Criticism would be easy. In many ways his work is dated and outgrown, though as he himself urges, the quest of earlier seekers for the truth must always be respected. It has to be remembered that his work belonged to a time when the philosophy of science was in its infancy, and that he was a pioneer, notwithstanding Herschel's small *Preliminary Discourse*. No doubt it was his *History of the Inductive Sciences* which was the most influential of his writings on science, and we have not made reference to that work here. Mill, who disputed with Whewell over the questions of necessary truth and over the nature of induction, expressed his gratitude for the material collected in that *History* : ' Without the aid derived from the facts and Ideas contained in that gentleman's History of the Inductive Sciences, the corresponding portion of this work would probably not have been written ' (preface to the first edition of his *System of Logic*).

THE EVOLUTION OF THINKING[1]

R. J. Pumphrey

Although this title may seem pretentious and to some, perhaps, even irrelevant, I do not think that Darwin would have been shocked by it. He wrote the prolegomena for a treatise on the subject in 1872 and he would, I think, rather have been shocked that the road he indicated had not been further explored. For here Darwin failed in his encounter with prejudice, a failure which he owed at least as much to his well-meaning friends as to his enemies. 'Man is an animal' said Darwin in effect. 'Certainly', said the friends, who had made it their task to put Darwinism across, 'Man is the reasoning animal, the self-conscious animal, the talking animal, the constructor of railways and ethics, the inventor of law and conscience and differential equations, the highest animal, the animal with free will.' Such comfortable words smoothed the passage of the evolutionary hypothesis, for it was seen that it had no personal application. The man in the street might have inherited his kidneys from his ancestors, but his mind at least was wholly his own. Rousseau was right in his opinion that man was born free. The shackles in which he was subsequently observed to be entangled were an unfortunate accident. Theoretically he was still safely in his Cartesian observatory, divinely privileged to observe the universe and all the other animals from outside. The wall which Descartes had erected between man and the animals was still there and apparently as solid as ever. But, in case it had been shaken by Darwinism, steps were taken to buttress it. Anthropomorphism was raised to the status of a heresy. The intelligence of animals was debunked by a number of striking public trials, such as those of the Elberfeld horses and Clever Hans. The canon of Lloyd Morgan was ruthlessly applied, and doubters were effectually silenced by the simple expedient of denying credence to any account of animal behaviour which could not be reproduced at will in the laboratory. These measures were effective and the wall remained stable until it was breached by treachery from within.

For although Freud may never have heard of Lloyd Morgan and perhaps regarded the Cartesian wall with indifference, yet, in effect, he used the canon of the one to blow a hole in the other. The canon runs as follows: ' In no case may we interpret an action as the outcome of the exercise of a higher psychical faculty if it can be interpreted as the exercise of one which stands lower in the psychical scale.'

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[1] Paper read to the Philosophy of Science Group, 12th January 1953.

This, if its meaning is not too closely scrutinised, seems to be a meritorious application of Occam's razor. But, in fact, it was simply used to cut the connection between man, intuitively aware of his own intelligence, and animals which, since they could not be questioned directly, could be said in any given circumstances to be simulating intelligent behaviour instinctively. It was held self-evident that instinct was ' lower in the psychical scale ' than intelligence. Intelligence, nowhere defined, seems to have been tacitly accepted as the capacity to think out the implications of action beforehand.

Freud's important contribution was to wreck these implicit assumptions by pointing out the very peculiar rôle of reason in human activities. For, in most cases where human behaviour seemed to an external observer to simulate intelligence (and to the subject himself to be intelligent), analysis showed it to be due either to habit or to an unconscious choice of alternatives for which a rational basis was found *subsequently*. The function of reason was shown to be the *post-facto* justification of the unconscious choice. Man must be able to satisfy himself as well as his neighbours that he has behaved himself in accordance with the rules in an environment which is also governed by the rules. I use ' rules ' here in the widest possible sense as the basis of expectation—that night succeeds day, that creatures behave according to their kind, that a fire warms, that mother will rescue one if one is lost or in difficulties. If the basis of expectation is shattered or radically altered, two results are possible. Reason may keep pace and alter the rules so as to maintain a system of belief consistent with itself. This may be called the rationalisation of the failure of expectation. Or alternatively, reason may fail to deal with the situation, in which case the result is a feeling of inferiority or the kind of psychic trauma called neurosis or hysteria.

Since neurosis is so clearly linked in man with inadequacy of that process of conscious reasoning which we call in common speech thinking, it seems to me profoundly significant that neurotic symptoms are so readily provoked in mammals, and probably also in birds and even insects, and that by methods which can be subsumed under the general heading of removal of landmarks. These may be actual landmarks, indicating to the subject that the familiar environment persists in its familiar relations, or they may be metaphorical landmarks—the rules of expectation governing familiar changes in the environment. The distinction is not absolute, and the effects of removal are similar. Changes of actual landmarks which make the animal feel lost, like Blake's little boy ; substitution of punishment for reward after the solution of a problem which the subject has learnt to solve correctly ; alteration of the conditions of an accustomed problem, so that a correct solution becomes impossible or purely chance : all these are standard methods which produce fairly standard results, though individual idiosyncrasy is marked as in the human case. It is important to realise that it is not the setting of an insoluble problem in itself which is an efficient cause of neurosis. It is essential first to create the expectancy that the problem can be solved. It is not the total obliteration of landmarks which is effective but their displacement, so that familiar guides, on which reliance has been learnt, prove to be misleading. It is necessary to breach the animal's confidence in the rules, to reduce him to literal or metaphorical bewilderment.

The symptoms are much as might be expected if the analogy with human behaviour is exact. Mild cases exhibit active appetitive or exploratory behaviour clearly directed towards re-establishing familiarity and creating new rules of expectancy, followed, if these efforts are unsuccessful, by passive waiting ' until this tyranny be overpassed '. Severe cases exhibit stereotyped postures and acts, which are not meaningfully related to the prevailing environmental circumstances and which persist long after the cause is removed. In such cases, learned accomplishments are generally lost, and even feeding and mating may be adversely affected.

Freud attributed a secondary unconscious purpose to the apparently purposeless manifestations of neurosis and hysteria—that of attracting notice and enforcing aid and solicitude. If he was right, this appears to be the only notable difference between the animal and human conditions. Even this may not be a real difference, for much of the behaviour of the neurotic animal may plausibly be interpreted as a return to ' infancy '—an attempt in the Freudian sense to reproduce the conditions in which mother will come to its aid.

I have suggested that reasoning or thinking is always related retrospectively to the solution or non-solution of a problem in the light of accepted rules and a consequent state of expectancy. It does not, of course, follow that because it is retrospective that it is therefore epiphenomenal and ineffective ; for all problems have something in common and most real problems have much in common. Consequently, retrospective thinking about a choice already made may be, and, in general, probably will be, effective in contributing to the next choice.

To my mind the evidence is decisive that animals think in the way that we do, and that both in animals and ourselves, thinking depends absolutely on the ability to accept experience in the form of rules, by which expectation is usually fulfilled. It is very easy within limits for us to impose new rules on an animal by the methods of conditioning and *Dressur*, but I think it is profoundly mistaken to suppose that the process by which the animal assimilates them is usually wholly automatic and passive. Even Pavlov realised that what he (or his translators) rather oddly called the ' curiosity reflex ' is prepotent. An animal transferred to a new environment has to satisfy himself of the new rules which are appropriate. The process by which he does it is partly assimilative but partly creative. He explores, but he also erects signposts. And not until he has re-established familiarity does his behaviour approach normal. The length and elaboration of this process in the rat is only now beginning to be recognised. An understanding of it explains some of the anomalies in the many experimental investigations of maze-running in rats, but it also, I think, throws doubt on the validity of many of the published conclusions.

It is sometimes asserted that man has a conscience and animals have not. I am using the word in its common speech sense and in this sense the assertion seems to me plainly false. A cat, normally fed on the kitchen floor, if it is clouted for taking food off the table, becomes for a time nervous about taking food anywhere in the kitchen. It has a bad conscience. An Airedale, which on neutral territory would bite a smaller dog in half, will fly in panic from a Pekinese if he meets the latter on his own ground, which is strange to the Airedale. In this case the Airedale has a bad conscience and the Pekinese a good one. Quite evidently a ' bad conscience ' describes the state of discovering that the accepted rules have let one down or that one has entered a region where they are no longer self-consistent. It appears to be exactly equivalent to a feeling of inferiority or inadequacy consequent on the non-fulfilment of expectation—of stepping, so to speak, on a stair which is not there.

I turn now from the conscience question, which deserves a much more careful analysis than I have time for, to counting, another aspect of thinking, and one which man has been inclined to regard as his own prerogative, in flat defiance of the evidence. Kronecker's dictum: 'God made the integers; all else is the work of man', seems to have perpetuated a belief that counting starts with a knowledge of the integers, and even Otto Köhler, who recently demonstrated in the laboratory that birds can count when suitably motivated, a fact long known empirically to wild-fowlers—even Köhler suggests that the process of counting by birds differs radically from the human operation. On the other hand, Piaget's work on the ontogeny of counting by children seems to imply that it depends on something much more fundamental than the integers. A child of four, who has not yet learnt to count by numbers, and who is shown a set of eggs and a set of egg-cups, has no difficulty in withdrawing an egg for every egg-cup until one or both of the sets is empty. This counting by one-to-one correspondence seems to be primitive. In a sense it is a special case of a use of correspondence, which must surely be common to all animals capable of sensory experience in more than one mode, since without it, it would not be possible to associate an appearance with the corresponding smells, sounds and so on.

The small boy counting egg-cups with eggs is not limited by the size of the sets, but he is limited in two ways, which seems to be significant: first, by the functional relation between egg and egg-cup. An apple or a match-box among the egg-cups is ignored even when the set of eggs has been emptied. He is counting egg-cups by means of eggs. He is not ticking off all discrete objects which are accessible. Secondly, there is the temporal limitation. The sets must be simultaneously present. He cannot tick off eggs today against egg-cups seen yesterday, though he may be able to say 'more' or 'less' if the size of the sets is substantially disparate and not too great.

To return now to the birds. Köhler, using pigeons, parrots, jackdaws and ravens, succeeded in establishing a one-to-one correspondence in the minds of the birds between the number of objects displayed to them in an arbitrary spatial arrangement and the number of discrete acts of swallowing pieces of food which was permissible at one sitting.[1] The correspondence was effective up to 5 or 6 for pigeons and up to 7 or 8 for ravens and parrots. It is an asymmetrical type of correspondence between sets of different kind, which has not, so far as I am aware, attracted the intensive interest of either psychologists or logicians, though it would seem to be of fundamental importance.

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[1] It was suggested in the discussion that Köhler's observations are explicable in terms of *eidetic imagery*. If this means that an *eidos* is associated with the fiveness of five objects irrespective of their size, shape and spatial arrangement, it seems to be an explanation of the more simple in terms of the more difficult. If not, it does not appear to be an explanation at all.

The asymmetry may be exemplified by reference to human experience. Suppose a man is asked to remove an egg from a set of eggs every time he hears a gong stroke. Provided the strokes do not succeed each other too rapidly, there is no difficulty and no limit except physical exhaustion. The whole pattern can be regarded as a one-to-one correspondence between commands and the execution of commands. All this is well known to be within the competence of many mammals, birds, reptiles and fish, though the similarity is often disguised by language, students of animal behaviour being prone to use the terms, sign-stimulus in lieu of command, release in lieu of execution, habituation or exhaustion of action-specific energy in lieu of inattentiveness or loss of interest.

Suppose, however, that a man is asked to strike a gong once for every egg in a set of eggs. It is immediately obvious that the task is impossible of accurate performance in the general case except by subterfuges, but it is important to see exactly why and to investigate the nature of the possible subterfuges. The set of gong strokes is a temporal set or series such that the ordinal number of the last member and the cardinal number of the set are equal. On the other hand, the set of eggs in a circumscribed region, say, on a table, has no ordinal numbers and it would seem impossible in principle to determine its cardinal number (if the latter is more than one) except by seriation, that is by taking the members of the set one after the other in time. It follows that the cardinal number of two sets A and B can be compared if, and only if, the members of both sets can be taken one at a time and paired off *more or less simultaneously*, that is by making both sets into corresponding temporal sets. Obviously by introducing a third temporal set, the set of integers, C, and by means of an adequate extension of memory, the condition of '*more or less simultaneously*' can be relaxed for the comparison AB since it can be executed by making the comparison AC now and the comparison BC in a week's time. But the condition still holds individually for the comparisons AC and BC. What meaning must we assign to *more or less simultaneously* if the comparison with the integer set is excluded? Evidently in the case considered the man may without difficulty take an egg from the set, walk across the room to strike a gong and repeat this cycle indefinitely.

Memory, both in man and animals, easily bridges the gap between a command and its execution unless more commands occur in the gap. But suppose the man takes eggs from the set *n* at a time and subsequently walks across to strike the gong *n* times, is there an upper limit for *n* above which inaccuracy becomes probable? An experimental answer should be attainable, but it is practically difficult to interpret if the subject is accustomed to counting by integers. Since, however, we have seen that counting by integers and counting by gong strokes are fundamentally similar processes, the following experiment is relevant. A random arrangement of objects in a circumscribed field is exposed to the subject's view for a period too short for him to count them. He is subsequently asked how many objects were exposed. It is found, provided that the objects are similar, and, by reason of size and contrast, sufficiently prominent, the answer is generally correct if the number of objects is not more than five or six. It follows necessarily that the operation of seriation can be performed upon the mental image or memory trace of the set of objects which has been exposed.[1] If we admit what seems to be self-evident, that the operation of counting is basically the same whether the comparison set is the set of integers or the set of gong strokes or the set of acts of swallowing peas, then the experiment just described—also due to Köhler—closely simulates his experiments with birds. It is perhaps of some interest that man's performance is about as good as a pigeon's but not as good as a raven's or a parrot's. The raven's performance is the more remarkable in that he can get the right answer even if the members of the set of object-signals are grossly dissimilar, and this ability to single out denumerability as the only property of the members of the set which is relevant to the situation seems to indicate a considerable facility for abstraction.

The peculiar usefulness of the set of integers lies in their being assigned proper names which determine uniquely their order and the place of each in the series. In most systems these names are arbitrary up to ten and then repeat with a systematic difference so that the name of any number, however large, can be constructed by simple rules. There seems to be some philological evidence for believing that *one* and *two* antedate the rest of the series, and there are, I believe, tribes which have only the three numerical adjectives, *one*, *two* and *many*. It seems a plausible guess that both counting by named integers and a system for constructing the names of large numbers originated in the first pastoral society at the point where it was becoming inconvenient to give a proper name to each individual member of a flock. A shepherd ticking off his flock as it came through a gap : *Agatha*, *Mary*, *Susan*, *Jane*, would be quick to realise that, if the total tally of names and sheep were equal, it did not matter *for this purpose* whether each sheep got its right name, and that a fixed order of names was easier to remember and later to transfer to a different set of sheep. Simultaneous ticking with fingers could introduce a periodicity of ten ; and the addition of determinatives indicating the decade and century seems a relatively trivial addition to the main invention.

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[1] J. S. Huxley drew attention in the discussion to the 'intention movements' used occasionally by jackdaws in checking, i.e. recounting the number of acts of swallowing without repeating them. This appears to fit exactly with the interpretation here adopted, and to show that seriation of the memory trace actually occurs in birds.

The suggestion that the names of the integers are buried names of sheep may not be true in any exact historical sense, and it is very unlikely that it could be proved to be true ; but it seems to me very probable that it is a true parable.

If I have laboured this business of counting to the point of tedium, it is for two reasons. In the first place, it brings out what is apparently an unexpected similarity in the working of the minds of birds and men, where most people have for a hundred years been concerned to demonstrate expected differences. It is a further breach in the Cartesian wall.

In the second place, the ability to perceive and retain the perception of a one-to-one correspondence between members of dissimilar sets, even when the correspondence is blurred by a lack of temporal coincidence, is the basis not only of numeration but of speech. The correspondence between things or acts and their names, by which the thing suggests the name and the name suggests the thing, is strictly one-to-one. The arithmetic of words is Diophantine, and fractions of words have no more use than fractions of egg-cups. Evidently this is not the only type of correspondence. The correspondence between sensory impressions in different modes, to which I have already referred, is quite obviously a correspondence of *continua*. The appearance and the sound and the smell of running water have no definite spatial and temporal boundaries. Sensory impressions in general are no more denumerable than the elements of an Irish stew. The ability to recognise, as significant, denumerable elements in the Gestalt is quite possibly a consequence of the possession of eyes and ears of high resolving power. We share these with most birds, but I hazard the guess that it may prove difficult to repeat Köhler's experiment with mammals other than primates and perhaps squirrels.

It seems appropriate at this point to emphasise two relevant facts. I think it is fair to call them facts, though the paucity of our knowledge is so extreme, that there is inevitably an element of conjecture.

So far as we know, the languages, or the communication systems of non-human animals, are based mainly on the correspondences of *continua*. Their symbols or sign-stimuli are not, in their employment, particulate like words, but are capable of infinitesimal gradation of meaning within their field of reference. The correspondence between symbol and meaning is approximate, and the approximation can, in theory at least, be made as close as is desired. This is evidently true of the system by which a bee, returned from foraging, indicates to her fellows the co-ordinates of a source of nectar. It appears to be true generally, so far as our very scrappy knowledge goes, of the employment of visual and aural symbols by all classes of vertebrates and some invertebrates. It would seem *prima facie* that it is of necessity true of odorous symbols, whose importance for most mammals is hardly questionable, however trivial a part odours play in our own lives. It is true of the human language of gesture (excluding arbitrary sign languages). The direction of an object can be indicated by pointing to any desired accuracy. Any degree of indifference can be signified by raising the shoulders to any desired height within anatomically determined limits. In this type of language the number of symbols and of the fields within which they are significant is necessarily very limited, but each symbol within its field is capable of an infinity of related shades of meaning. In a ' purely ' verbal language, on the other hand, although the number of symbols is theoretically infinite, there is a one-to-one relation between each symbol and a corresponding meaning or group of meanings, and it is consequently impossible in principle by their use to approach indefinitely close to accuracy in the transmission of information. This defect of particulate systems crops up in other connections, in cybernetics and in quantum theory. A case of some interest is the transmission of messages in sensory nerves, where it is easily shown that as a consequence of the particulate nature of the message, which consists of discrete and equivalent impulses separated by variable intervals, the parameters of a stimulus can be transmitted only with indefinite accuracy in an indefinitely long time. The information received at the central end is necessarily both inaccurate and out-of-date, and its possible accuracy is inversely related to its possible immediacy. Similarly it may be said of any purely verbal statement that it is either too short to be accurate or too long to be useful, but here the limitation is practical. If it is true in theory that indefinite accuracy of verbal statement could be attained by an indefinite increase in the length of the statement and in the number of different words used, it is nevertheless also true in fact that the average man can only deal with a vocabulary of a few thousand words and of these a very large proportion are used so infrequently that their ambiguity is large.

Spoken human languages are, of course, not purely verbal systems. The particulate words are associated with intonation which is capable of continuous variation and so of infinitesimal gradation of meaning, and are often supplemented by gesture of which the same is true. Such a balance of two systems of communication might be expected to compensate for the defects of either and to some extent it does. But its usefulness is limited by the impossibility of transcribing anything but the purely verbal component. The inventions of writing and printing have dragged the balance down on the verbal side. It remains to be seen whether radio and television will restore it, but I suspect it is too late. Five thousand years is a long start.

It seems worth while, therefore, to glance briefly at the way in which the defects inherent in purely verbal systems have been evaded or overcome. It is a field in which I make no claim to be expert or even to be informed above the average, but certain general propositions seem worth stating if only for the purposes of argument.

The problem facing the users of a verbal language in a changing environment is to invent new words and to enlarge or change the meaning of old ones without introducing more than a minimal amount of ambiguity. It is, I think, obvious that definition is no solution. Definition, as Lytton Strachey pointed out in connection with the Thirty-nine Articles, is only effective and, indeed, only possible when there is already agreement among users about the meaning of the term to be defined. Definition is the crystallisation of an accepted meaning; it is not the road to it.

I hazard the conjecture that new words and new meanings (not only those which are onomatopoeic) invariably arise by analogy. The process involves an operation, or sequence of operations, which may be explicit or implicit and is equivalent to saying: *X* (the unknown) is like *A*, is like *B*, is like *C*, etc., where *A*, *B*, *C*, are terms of accepted meaning. The Kingdom of Heaven is like a grain of mustard seed, is like a lamp hidden under a bushel, is like the eye of a needle, is like a man that is an householder. The emergence of new meaning is evidently dependent on the co-operation of speaker and auditor, writer and reader : 'They that have ears to hear let them hear'. Anyone who attempts like Humpty Dumpty to be the master of words and to inflict new meaning on his readers condemns himself to perpetual misunderstanding. I have taken a case where the analogy is explicit, but a cursory hunt through an etymological dictionary discloses how very few of the terms of, say, physics, are arbitrary, and how many derive by more or less extended analogies from roots which are connected with personal and intimate acts and entities and which are therefore presumably very old : *distance* from 'standing away from', *interval* from the space between two hedges, *mass* from a lump of dough and ultimately from the act of kneading, *time* from stretching. All such terms can be truly regarded as buried parables or metaphors or analogies, and I do not think it is too far-fetched to trace a resemblance between the correspondences by which the meaning of words is extended and the correspondences by which things are counted ; or to maintain that analogy is a proper description of the process by which such correspondences are detected in both cases.

I now venture on even more dangerous ground. I assert the proposition that the behaviour of animals and men expresses a fundamental conflict which I should be willing to call dialectical if I was sufficiently sure of what is meant by that word. The conflict is between the tendency to abide by the rules of expectancy and the tendency to increase the region in which the rules can be applied. Each venture into new territory involves the risk of the unexpected and the consequent necessity of a new synthesis of the rules. Each failure to venture preserves the rules at the expense of freedom. This is perhaps platitudinous in its application to man. I merely point out here that it appears to apply with equal exactness to the territorial adventures of a dog or a robin and to the most exacting intellectual adventures of mankind.

I also assert the proposition that the distinction between instinctive and intelligent actions is misleading, and that the creation of a hierarchy of psychic faculties in which intelligence is placed above instinct is artificial and false. Instinct is the inherited framework within which behaviour is possible ; intelligence is the degree of freedom within that framework. Again, so far as man is concerned this may seem a platitude. None of us would, I presume, admit that his sex-life was wholly instinctive, but surely none would claim that it was wholly rational. I merely point out here that the stereotypy of instinctive actions by animals is largely the result of the tendency of human observers to abstraction and selective description. It is convenient for classificatory purposes to regard all horses as equal, but no user of horses makes the mistake of supposing it to be true. And no accurate observer of behaviour believes that a similarity of instinctive actions sufficient for group classification implies identity. It is indeed obvious that some kinds of instinctive actions appear more hidebound than others ; it is true that for some species of animals the whole inherited framework is stricter and the degree of freedom less than for others. This corresponds to the rather old-fashioned distinction between animals which are adapted to their environment and those which are adaptable. But there is no animal which is not in some degree adapted and none which has wholly lost adaptability. There is no absolute distinction between the bound and the free. There are only different degrees of freedom.

As I have tried to show, human speech is a product of a conflict which at once enlarges the field of possible action and restricts action by the necessity of compliance with rules on which intelligibility depends. Intellectual progress depends on tampering with the rules so tactfully that ambiguity is minimal. But some ambiguity there must be. No experimenting with the rules means stagnation. Too vigorous an attack results in unintelligibility. And, as in the case of so-called instinctive acts, the framework of rules is common to a group, but the tampering is individual and unique.

This is I think a point of real importance. I suspect it accounts for the one-sided treatment which animal behaviour has received. Respect for the controlled experiment and the statistically significant result has concentrated attention on the rules to the exclusion of the exceptions. Masses of statistics about the mean Wistar rat are available. Their validity is in any case dubious because, as Haldane and Spurway have pointed out, the Wistar rat is not normal, but, if it were, it would still not be the mean rat but the exceptionally talented and adventurous and adaptable rat, the leader and trail-blazer who is interesting. And to him, statistics about the mean rat are about as relevant as an I.Q. examination of the population of this country at the accession of William and Mary would have been to an estimate of Isaac Newton. There is some relevance but not much. Newton was undoubtedly bound by the intellectual climate of his time, but it was his marginal freedom which was important and significant. The concept of the mean rat has its uses ; but to mistake an actuarial convenience for the reality is a fantasy of the same kind as may tempt us to make a deal with the average Russian, or to imitate the American Way of Life.

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NOTES AND COMMENTS

Relativistic Quantum Theory

Prof. E. Schrödinger has kindly allowed us to publish the following extract from a private letter regarding a possible discussion on Relativistic Quantum Theory.

It is certainly interesting to discuss the basic aspects of the two theories : special theory of relativity and quantum mechanics. It seems to me that there is still a fundamental incongruence between them, in spite of all the trouble that has been taken in various branches of quantum mechanics to ' put it all into Lorentz-invariant form '. If I were asked to point out the incongruences in a general way, I think I should mention two things. *First*, in laying the foundations of special relativity, one has continually to make ' thought experiments ' (*Gedankenexperimente*) with clocks and measuring rods assumed to be at rest or in a definite state of motion with respect to each other while in the same consideration any clock and the ends of the measuring rods are ascribed definite values of the co-ordinates, and thus definite locations in the frame that is used. But according to the ' principle of indeterminateness ' of quantum mechanics this cannot be done, or it can only be done approximately for fairly heavy clocks and measuring rods. This would seem to restrict special relativity to the macroscopic domain, which is very unsatisfactory ; moreover, special relativity does prove valid when applied to atoms and even to elementary particles, for instance, in considering the Doppler-effect of atoms or the time scale of decay of highly energetic mesons passing through the atmosphere (in the latter case the relativistic change of the time of decay is not a small effect, it means a *factor* of the order of 50 to 100).

Secondly, the current probability-interpretation of quantum mechanics consists in a prescription for computing the probability of finding a certain state of affairs *at a given time*. The said prescription has to make use (by integration over space) of the values of a certain function of the co-ordinates and the time *at that time*, i.e. of the simultaneous values of this function. In another Lorentz-frame—since the Lorentz transformation involves the co-ordinates *and* the time—two things happen ; first the whole store of probability questions that the prescription answers is changed, because what is, e.g. a given region at a given time in one frame is no longer a given region at a given time in the new frame ; secondly, the store of ' simultaneous ' values of that function (of the co-ordinates and the time) that the prescription uses for computing the probabilities is a different one in the new frame, different not just by a local transformation, but because the space-like cross-section (of the space-time manifold) on which the said function has to be considered is entirely different in the new frame—as different as two planes intersecting each other at a finite angle. This makes one doubtful whether the two sets of answers to the two sets of probability-questions are always fully equivalent, as they ought, of course, to be. However, I know that much thought has been devoted to this question in the last years, and also that I am not too well informed about it.

Dirac's relativistic wave equation still stands out as *the* great success that has been scored in this whole subject. I believe that for Dirac's equation the ' probabilities of location ' are consistent in all frames, and this as a consequence of the ' equation of continuity ' between the four quantities that correspond to the four-current and may, in any frame, be interpreted as probability of sojourn within an element of volume, and probabilities of passage through certain elements of surface within an element of time.

E. SCHRÖDINGER

E. A. Milne's Scales of Time

MR G. W. SCOTT BLAIR's revised comment on E. A. Milne's τ and t scales of time[1] prompts me to explain why I regard Milne's influential interpretation of the relation between these two time-scales as erroneous and why I think that Mr Scott Blair's original comment on them[2] is less misleading than he now supposes it to be.

Milne's insistence on the unique importance of the logarithmic relation between the two time-scales[3] derives from his having interpreted that relation to say that the physical world could not have existed at times corresponding to *negative* values of t, that is, prior to the onset of the expansion characterising the present epoch. Thus he writes : ' . . . relationships, containing . . . mention of the epoch t

[1] G. W. Scott Blair, this *Journal*, 1953, **3**, 359

[2] G. W. Scott Blair, this *Journal*, 1952, **3**, 82

[3] Cf. E. A. Milne, *Kinematic Relativity*, Oxford, 1948, pp. 222-233

to which they refer, bear witness to the creation of the world in time, taking due account of the actual origin of things '.[1] But rigorous scrutiny of the mathematical justification which he offers for this claim discloses at once that Milne's philosophical interpretation of his own important metrical discovery is specious. To be sure, the relation

$$\tau = t_0 \log \left(\frac{t}{t_0}\right) + t_0$$

does tell us that when t approaches 0, τ decreases indefinitely *and* that for $t \leqslant 0$, τ is *not defined*. Speaking *elliptically*, we could say with Milne that ' $t = 0$ corresponds to $\tau = -\infty$ '.[2] Do these mathematical facts warrant the conclusion that $t = 0$ marks an absolute beginning or creation? As I have pointed out elsewhere,[3] this philosophical interpretation can be obtained only by reasoning fallaciously with Milne and T. Page that ' one could hardly expect *more* than infinite time on the clock [τ —] scale '.[4] For the concept ' more ' cannot be applied in this finitary fashion to $-\infty$, which is *neither a number nor a possible value of* τ. (Page's reasoning would be equally invalid even if $\aleph_0$ were a possible value of τ, which, of course, it is not.)

Let it be granted that Milne's logarithmic time-scale relation is correct mathematically. If my criticism of Milne is correct, then Mr Scott Blair can justifiably endorse Milne's claim that the two scales differ ' far more fundamentally than by a mere change of units and of zero ' [5] only by supplanting Milne's philosophical interpretation of the relation between these scales. I would like to suggest that the correct philosophical interpretation takes the form of a three-fold assertion as follows: (1) It is *not* an *empirical fact*, as had been previously supposed, that the metric of time defined by the earth's rotation has a time-invariant linear relation to the temporal metric defined by atomic clocks; (2) The τ-scale is useless, in the manner of a mathematically ill-behaved function, for purposes of giving a metrical description of such physical processes as may have occurred at times corresponding to *negative* values of t; and (3) the mathematical relation between the two time-scales *leaves open* the question of the physical meaning of negative values of t and of the existence of the physical world prior to the beginning of the expansion at $t = 0$.

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[1] Cf. E. A. Milne, *Kinematic Relativity*, Oxford, 1948, p. 232.

[2] *Ibid.*, p. 37

[3] Cf. A. Grünbaum, ' Some Highlights of Modern Cosmology and Cosmogony ', *The Review of Metaphysics*, 1952, **3**, 489–491

[4] T. Page, ' The Origin of the Earth ', *Physics Today*, 1948, **1**, 12

[5] E. A. Milne, *Modern Cosmology and the Christian Idea of God*, Oxford, 1952, p. 95

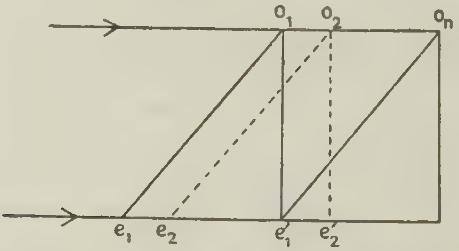
ADOLF GRÜNBAUM

Mr Dobbs' Two-Dimensional Theory of Time

I SHALL discuss the thesis of Part I of Mr H. A. C. Dobbs' article on 'The relation between the Time of Psychology and the Time of Physics', which appeared in this *Journal* in August 1951. Dobbs claims (1) that the phenomenon of the so-called 'specious present' involves a paradox; and *either* (2) that this paradox can be resolved *only* by postulating 'a second dimension in the extensional aspect of time'; *or*, the weaker claim, (3) that this paradox *can* be thus resolved. I am not certain whether Dobbs wishes to make claim (2) or merely claim (3), but the stronger claim is the more interesting one, and it is clearly indicated by some of the things he says, e.g. in the last paragraph on page 136. I wish to challenge each of these claims.

Concerning claim (1), the paradox which worries Dobbs seems attributable not to the phenomena—the feature of our experience to which the concept of the specious present is applied—but to the language in which Dobbs chooses to describe the phenomena; and, in particular, to the jump he makes from saying (i) 'within a single specious present, there may be event-components which, in one way, are manifestly successive; yet which are, in another way, equally manifestly, all *compresent together as a whole*', to saying (ii) 'in other words, in a speciously present experience, manifestly successive event-components can also nonetheless manifestly be *contemporary in time*' (pp. 124-125, *my italics*). Now the two predicates which I have italicised are not equivalent. The first statement does not entail the second, and it is the second which, for Dobbs, constitutes the paradox. Consider one of his examples—hearing the postman's 'rat-tat'. Here, the two noises are not (in the familiar sense of the words) contemporary in time, though there is an important sense in which they can be said to be 'compresent together as a whole', if we mean that we hear the 'rat-tat' as a gestalt. In such a context 'compresent' presumably means 'presented (perceived or apprehended) together', and it seems gratuitous to interpret it as meaning 'occurring simultaneously (from the viewpoint of a second time-dimension)'. Dobbs, however, deems it necessary to postulate a second time-dimension (' transition-time ') in which the ' rat ' and the ' tat ' are simultaneous, and this is said to be orthogonal to ' phase-time ', the time-dimension in which the ' rat ' and the ' tat ' are successive. But this is to ignore the more orthodox ways of representing spatially the specious present phenomenon. According to the latter, to say that O's awareness of event *a* is simultaneous with O's awareness of event *b* does not entail that *a* and *b* are themselves simultaneous. This point is also overlooked by Mr G. W. Scott Blair[1] who writes, in his discussion of Dobbs' article, ' certainly if we spatialise time we must accept two dimensions for it '.

Concerning claim (2), presumably Dobbs wishes us to interpret his theory as Professor C. D. Broad does in the logistic analysis appended to his article. In this appendix, under the (scarcely appropriate) heading ' Empirical facts about normal specious presents ', Broad deduces a number of theorems from the assumptions of Dobbs' theory. The briefest way of disposing of claim (2) is to show that the relevant features of our experience can all be accommodated equally well (and, indeed, much more simply and intelligibly) if we interpret the specious present by means of a spatial model involving only one time-dimension. And, in fact, for each of the theorems which Broad deduces from Dobbs' assumptions, a corresponding theorem, which has precisely the same ' cash-value ' can be deduced from the assumptions made by Broad, in his analysis of the specious present in *Scientific Thought* (pp. 348 seq.). The diagram used there is reproduced below.



Broad interprets this diagram in the following way. The lower horizontal line represents a ' slice ' of the history of an observer's ' sensible-fields ' ; the upper line a ' slice ' of the history of his ' acts of sensing ', the apex of a triangle representing a ' momentary act of sensing '. The direction left-to-right represents earlier-to-later, and position on either line represents date—in one and the same time-dimension, it being assumed that a momentary act is simultaneous with the later boundary of its content, i.e. of the ' sensible-event(s) ' apprehended by it. The length of the base of a triangle represents the duration of the content of the corresponding act. This duration is identified with that of the observer's specious present, and Broad treats it as having a constant value, τ. (This last assumption is made presumably to simplify exposition. Broad would, I am sure, allow that the duration of a person's specious present may vary, depending, for example, on his interest in, or experience of, the kind of events being observed.) The implications of this model are perhaps most easily grasped, if we interpret the diagram by analogy with a searchlight which moves along the upper line and illuminates a (constant) span of the lower line.

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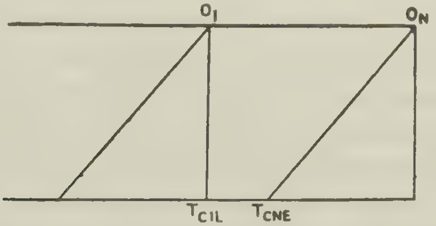
[1] This *Journal*, 1952, **9**, 83

It seems unnecessary to restate in terms of the simpler model each of the theorems which Broad deduces from Dobbs' assumptions. I shall, for purposes of illustration, consider two of the most complicated theorems in Broad's appendix, namely (1.2) and (2.2). Each contains three propositions, with a diagram for each proposition. I shall give the ' translation ' of (*a*) the first proposition of (1.2) and (*b*) the second proposition of (2.2).

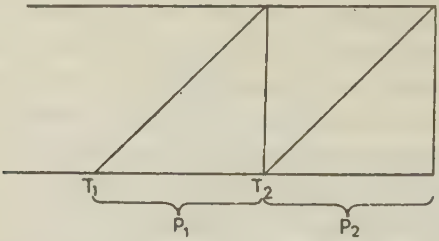
(*a*) Let T_{01} = the date of the momentary act O_1 ;
T_{ON} = the date of the momentary act O_N ;
T_{C1L} = the date of the later boundary of the content of act O_1 ;
T_{CNE} = the date of the earlier boundary of the content of act O_N.

There is a certain duration, τ, such that,
if T_{ON} *minus* $T_{01} > \tau$, then T_{C1L} precedes T_{CNE}.



(*b*) Let P_1 = the period throughout which a momentary event occurring at T_1 is presented ;
P_2 = the period throughout which a momentary event occurring at T_2 is presented.

There is a certain duration, τ, such that, if T_2 *minus* $T_1 = \tau$, then the later boundary of P_1 coincides with the earlier boundary of P_2.



Anyone who cares to compare the foregoing with what is given in Broad's appendix will, I think, grasp the relationship between the two models and find no difficulty in translating the other theorems. I suspect that the theorems given by Broad in this appendix were first worked out by means of the simpler model. At any rate, if we use Dobbs' model, the theorems can scarcely be grasped by elementary spatial intuitions, as they can be if we use the simpler model. Consider my theorem (*b*), and think of the diagram in terms of the moving searchlight. All that is involved is that if the distance (= duration) between T_1 and T_2 is equal to the width of the illuminated area on the base-line (= the duration of the observer's specious present), then the last moment at which something at T_1 is illuminated (= presented) coincides with the first moment at which something at T_2 is illuminated (presented).

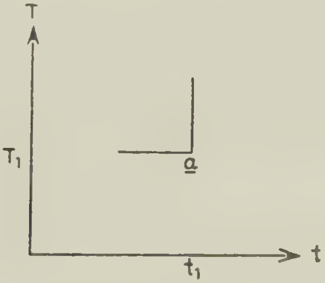
Now does Dobbs' model entail any theorems which (*a*) are not entailed by the simpler model, and (*b*) might have some ' cash-value ', i.e. might be confirmed by introspection, observation or experiment ?

This has not been shown. So far as the normal phenomena of the specious present are concerned, the answer must, I think, be negative. There are, however, some phenomena which may seem to require us to postulate a second time-dimension ; notably the apparent occurrence of precognition, or, more generally, the ' temporal displacement ' (in both temporal directions) disclosed in the results of several independent experiments in ' extra-sensory perception '.[1] Professor Broad once proposed[2] that we try to explain precognition by postulating a second

[1] See, e.g. the experimental reports by Whately Carington and S. G. Soal in *Soc. for Psychical Research Proc.*, 1940-41, **46**; 1942-45, **47**.

[2] *Aristotelian Soc.*, 1937, supp. vol. **16**

time-dimension. The point of his proposal was to suggest that although an event *a* precedes an event *b* in the familiar time-dimension, *b* might precede *a* in an unfamiliar time-dimension ; and in view of this *b* might intelligibly be said to determine the occurrence of *a*, e.g. in cases where *a* is a precognitive impression and *b* the event which confirms *a*. Professor H. H. Price has commented that this proposal would commit one to the puzzling notion of a ' double-now '. Dobbs quotes this remark by Price in Part II of his article, and writes ' no such idea is implied by the theory I am seeking to put forward. Such a double-now conclusion rests essentially upon a confusion between the "extensive" aspects and "becoming" aspects of time. Once the distinction is properly grasped, there is no . . . reason to think that *two separate* fluxes of becoming must flow independently . . .' (p. 284). In view of this, Dobbs is committed to saying that if *a* is a momentary sensible event which ' becomes ' (occurs) at t_1 (an instant of ' phase-time '), then T_1, the earliest moment (of ' transition-time ') at which an observer can begin to apprehend *a*, is *simultaneous* with t_1. This important ' axiom ' is not included in Broad's analysis of Dobbs' theory. To complete Broad's analysis, it is necessary to specify how the horizontal and vertical lines, which Broad shows in separate diagrams, are related, viz.



Here the horizontal line terminating in *a* would represent the phase-time duration of the content of an act occurring at moment T_1, and the vertical line would represent the transition-time duration throughout which *a* is presented. Now this new axiom prevents Dobbs' theory being used to explain precognition, for it implies that if any event is future in one time-dimension, it is future in the other. Moreover, the new axiom makes Dobbs' model more similar to our simpler model than appears in Broad's appendix, for this axiom corresponds to the assumption that a momentary act of sensing is simultaneous with the later boundary of its content—the assumption that is reflected in the use of the right-angled triangle in the simpler model.

In view of the new axiom, just what does Dobbs' model involve that is not involved in the simpler model ? So far as I can see, only this :

(i) that ' transition-time ' and ' phase-time ' are to be described as ' orthogonal '. (But is this more than to recommend an odd and awkward convention for drawing diagrams ?) ;

(ii) that different time-scales (temporal units) must be used to measure (*a*) the duration of sensible events and processes, and (*b*) the duration of acts or processes of sensing. The only reason I can think of for saying this is the phenomenological fact (not mentioned by Dobbs) that one's subjective impression of the duration of an experienced event may vary to some extent independently of its duration as measured by, e.g. clocks. This phenomenon can, however, be explained without invoking a second time-dimension ; for example, by saying that when two observed processes, equal in duration by clock-time, nevertheless *seem* to one unequal, this is because one is more interested in the process which seems longer, and therefore attends to more of its details, i.e. one *does* more, in the same time, in one case than in the other. I conclude then that Dobbs' two-dimensional theory of time explains no psychological data which cannot be explained on a conventional theory.

Regarding claim (3), I want to ask whether Dobbs' theory is really intelligible ? Dogmatism would be out of place here. One cannot be sure that such a theory will not in future prove fruitful for some purpose. I wish, however, to raise briefly some questions which call for an answer. Dobbs assumes that his ' transition-time ' and ' phase-time ', although orthogonal and involving different scales of measurement, can nevertheless be treated as different dimensions of a single determinable called ' Time '. What are the kinds of events which are supposed to be ordered in each of these dimensions ? Concerning ' phase-time ', Dobbs' answer is explicit—' sense-data ' or ' experiential events ', *not* the physical events whose existence and structure can only be known by inference (pp. 126-129). Then the events which are ordered in ' transition-time ' must be acts or processes of sensing (apprehending) as distinct from the sense-data which form their contents. Now what precisely is *meant* by talking about ' acts of sensing ', and by saying that the dates or durations of these must be measured on a time-scale different from the one which is applicable to their contents? Does Dobbs, or anyone else, claim to be able by introspection to discriminate an act of sensing from its content and to measure independently their respective durations? If not, and if it is admitted that acts of sensing are theoretical constructs, what theoretical purpose do they serve? I confine myself here to asking questions. Elsewhere[1] I have tried to answer them, and have argued that the concept of datable acts of sensing cannot fulfil the theoretical purpose for which it was introduced. If I am right in this, we must reject the model used by Broad in *Scientific Thought* as well as Dobbs' model. We can, however, still make sense of the phenomena of the specious present by adopting alternative metaphysical assumptions—and without invoking more than one time-dimension.

C. W. K. MUNDLE

Geodesics and the Space and Time of Physical Observations

DOUBTS have frequently been expressed regarding the relation of the geodesics of the 4-continuum of General Relativity Theory to the rods and clocks and measured lengths and times of physics. It is a blemish in this theory that the possibility of systematic measurements depends on the weakness of the gravitational fields and that very few problems can be formulated in tensor notation. Though these issues have already been considerably discussed one has the impression that they cannot be fully resolved until the wider question of the relation of General Relativity Theory to Quantum Theory has been answered by some future theory covering both realms. In the meantime the matter remains of great philosophical interest.

A. N. Whitehead doubted the possibility of measurement in a heterogeneous continuum and his Relativity Theory of Gravitation has recently been examined by J. L. Synge[2] in a most interesting paper. Sir Joseph Larmor expressed a similar doubt and his views are summarised in the following hitherto unpublished letter, which is reproduced here as his relevant publications are not easily accessible. I had sent him a paper on a point which he had treated thirty years earlier :[1] the necessity for a non-classical definiteness of linear scale (e.g. a natural constant of length) in any theory of atomic structure.

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[1] 'How specious is the "Specious Present"?', *Mind*, 1954, **63**, 26.

[2] J. L. Synge, 'Orbits and Rays in the Gravitational Field of a Finite Sphere according to the Theory of A. N. Whitehead', *Proc. Roy. Soc.* (A), 1952, **211**, 303. See E. T. Whittaker, *History of the Theories of Aether and Electricity*, Edinburgh, 1953, vol. 2, p. 174.

HOLYWOOD,
Co. Down,
27th September 1929.

Dear Mr. Whyte,

I have received your offprint on the necessity for a law of linear scale of the universe—with thanks.

Nay more—the fourfold has no means of expressing distance at all, so far as I can understand ; thus geodesic curves or any other curves traced in it can hardly be brought into direct correlation with movements of bodies, whether one calls them ' orbits ' or not.

I have tried to prove that the Einstein verified optical tests follow from the covariance of gravitation for all observers, without any reference to the seductive but illusive fourfold and its tensor theories, in a final Appendix to *Math. and Phys. Papers*, Vol. II,[2] which is accessible at the London and French and American Societies' libraries. That arises from the simplicity of the cases concerned. I do not believe that the fourfold tensor scheme has any effective contacts at all with the world as exhibited in the space and time of various observers themselves describing orbits ; which shows how far I am from salvation.

With warmest thanks,
Yours very truly,
J. LARMOR.

L. L. WHYTE.

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[1] J. Larmor, *Aether and Matter*, Cambridge, 1900, pp. 189-193

[2] J. Larmor, *Mathematical and Physical Papers*, Cambridge, 1929, Vol. 2, Appendix 6, p. 768.

REVIEWS

METABIOLOGY

'I TAKE it to be the primary business of a book to please and interest the reader', says Professor Woodger in the Preface to his Tarner Lectures.[1] His book has a peculiarly pleasant quality, perhaps because, despite the abstract nature of the subject, the author talks informally—which is not at all the same thing as colloquially. Some parts are simple and some difficult. Perhaps few readers will be interested in all of it, but every philosopher of science will find a great deal to attract him. It is specially addressed to those who are interested in *statements* about organisms as well as in biology itself.

The contents cover a wide field which cannot be indicated in a word or two but only described bit by bit.

In Lecture I, Professor Woodger is largely concerned with the nature of observation-statements and with matters of logical analysis. It contains an attack on the obscurity of the term 'property'; he prefers for some purposes a language that does not contain it—his preference for a language consisting of shared and unshared names will be familiar to readers of this *Journal*. This point concerns genetics closely; for Woodger considers that the theory of genetics cannot be expressed clearly so long as the concept of 'property' is used without our being alive to its pitfalls, and that in fact talk about 'heritable characteristics' is so vague as to be almost meaningless or else—whenever 'heritable' and 'acquired' are regarded as mutually exclusive—quite false.

Lecture II is about theoretical (explanatory) statements. One of the attractive things here is his way of distinguishing generalisations, which he calls zero-level hypotheses, from other theoretical statements, which he calls explanatory hypotheses of the first level. Zero-level hypotheses are formed simply by generalising an observation statement, such as 'this adult mammal has a heart', into 'all adult mammals have hearts'. First (and higher) level hypotheses cannot be obtained in this way. Woodger is further concerned with the logical aspects of the hypothetico-deductive procedure for using explanatory hypotheses. At the end of each Part the author gives sections of notes and appendices. Here he gives a superb twenty-page methodological analysis of Harvey's work on the circulation of the blood. The different types of statement are carefully labelled zero-level, first level statements, or consequences, and the entire theory is briefly expounded in a way that shows how excellent was Harvey's treatment of the subject. The book is worth having for this alone.

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[1] *Biology and Language: An Introduction to the Methodology of the Biological Sciences including Medicine*, J. H. Woodger (The University Press, Cambridge, 1952. Pp. xiv + 364. 40s.).

From general methodology we pass to Part II, which is said to concern methodological problems in genetics. It consists of mathematical applications of the Boole-Frege type of logic, in a widely used form of symbolisation developed by Woodger himself to include the biological constants. This language system is designed to make the fundamentals of genetics explicit, to eliminate the woolliness that he ascribes to the usual formulations in genetics, to reveal the structure of genetical theory by means of methodological analysis, to show how Mendel's results can be generalised, to make explicit for the first time several levels of explanatory hypotheses and show the relations between them, and thus to give a novel classification of genetical systems (with the ultimate aim of axiomatisation). He regards such work as of great importance for the proper understanding and also for the further advance of genetics. Many readers will find it difficult to use or even to follow, but it should certainly be given careful consideration.

Part III deals discursively with topics varying from the methodology of neurology and the mind-body problem to the training of medical students. These topics are too loosely knit to be described briefly, but they make interesting reading.

Of the many themes that are discussed or arise out of Part II of the book I now propose to dwell on the following : (i) the rôle of Woodger's symbolisation ; (ii) some precise restatements in English of his technical symbolic definitions in genetics ; and (iii) some further definitions that appear to be needed, arrived at by building on his work.

(i) There are many reasons for introducing a complex symbolism. It is done where it would be still more complicated to handle a given field without it. It is done in an abstract field where unaided intuition falters. It is done where it is likely to lead to consequences or further developments that would otherwise be almost impossible to obtain. It is done where it leads to interesting new theorems. Woodger claims all of these for his symbolisation, except the last. Blended with this, however, is the make-up of some researchers, of whom Woodger seems to be one, who find that they are so constituted that they have a penchant for working in this way. Clearly we must distinguish the situation in which a researcher finds such a method congenial for personal reasons and the situation in which results cannot otherwise be obtained by anyone. If we consider the case of Newton, he worked out some of his discoveries in one symbolism, but presented them to the public in another. Thus the methods used for discovery may be arbitrary and unlikely to be useful to others. Woodger evidently thinks that his methods can be used by others, and also that certain advances can probably be made in no other way. Of this I feel doubtful but at least the point ought to be discussed. Further, the consequences

Newton drew from his law of gravitation cannot be obtained without some mathematical method. Some at least of Woodger's, however, seem to be quite independent of their mathematical scaffolding : that is to say, even though some workers may need this scaffolding to attain their results in the first instance, we do not need it for reaching them, once we are shown what the results are. On the question whether this symbolisation is more likely than other methods to lead us to make discoveries, I would say that the inspiration, the idea must come first—as indeed Woodger would agree—and the symbolisation second. But, granted this, it is then merely a question of tactics whether one most easily works out the details or the consequences by means of some symbolisation or by some other means.

I am indeed doubtful about the intrinsic relevance of Woodger's symbolisation. I am even inclined to think that he would be more successful in persuading other scientists to try out seriously what he finds a powerful tool if he showed his readers how to give precise formulations in a more familiar way. However, he has made a bold new effort and it should be carefully examined, discussed, and tried out.

(ii) Of several loosenesses that Woodger appears to find in most statements of genetics, one is that they contain only an implicit reference to an important factor, such as the environment, which should be made fully explicit. Thus one gets the impression that a hereditary property is one that will develop no matter what the environment may be ; but, of course, as Woodger points out, it would not develop in strong sulphuric acid. The other main concepts involved are the thing from which something develops, i.e. the zygote, and the thing into which it develops, i.e. a member of a phenotype. Thus, while most ordinary formulations mention only two of these, Woodger is emphatic that confusion must result unless all three are kept explicitly together. With this one must agree ; and he has surely taken a fundamental step in the clarification of the subject. To achieve his ends he introduces the concept of ' environmental range ' associated with phenotype. It is this concept more than any other that seems to me to have illuminated the whole subject. One should add that it is an ' extensional ' notion and one that only a worker trained in mathematical logic would be likely to hit upon (which shows that a training in logic can be of value, though it does not prove that a technical symbolism is the most appropriate way to present a piece of work).

In order to show both that some at least of Woodger's definitions can be presented without the symbolic apparatus and also to show the power of his concepts, such as that of environmental range associated with a phenotype, I will now give a few of his definitions which I have translated into English :

> The ' environmental range ' of a set of zygotes with respect to a certain phenotype is the set of all environments in which the members of that set of zygotes develop into members of the phenotype (p. 130).

> The ' adult-environmental range ' of a set of zygotes is the set of all environments in which a zygote belonging to the set will develop into an adult (p. 183).

(Thus if the adult-environmental range of a set of zygotes is contained in the phenotype-environmental range, the environment need not be taken into account in genetical investigations.)

> A phenotype is ' environmentally insensitive ' with respect to a set of zygotes, if, and only if, the adult environmental range of the zygotes is included in the environmental range associated with the phenotype (p. 187).
>
> A phenotype is ' environmentally sensitive ' with respect to a set of zygotes, if, and only if, some members of the adult-environmental range do *not* belong to the environmental range associated with the phenotype (p. 187).

The last two are especially important : for Woodger they obviate the concept of heredity ; for me they open up a way of giving a precise account of this concept.

(iii) Can we now say in relatively simple terms what is meant by ' heredity ' ? In order to do this I would urge the restoration of the concept of ' property '. Woodger regards it as misleading, for it suggests that an abstract entity like redness, for instance, or even a concrete object like red hair may be handed down like a family heirloom from parent to offspring. No doubt scientists use language that implies this kind of mistake ; but are they ever misled by it to the extent of drawing false consequences from it ? If we take the risk of this, we may utilise one of Woodger's ideas in order to define ' hereditary property ' : we may say that a property is hereditary if, and only if, the phenotype whose members possess it is environmentally insensitive.

The main reason for restoring the concept of ' property ', however, is this. Woodger suggests (p. 188) that environmental insensitiveness may provide the meaning of heredity in some contexts. Now this identification seems to be satisfactory only so far as properties are concerned but to be inapplicable to many situations where it is reasonable to use the concept of heredity : for there are cases where the phenotype is environmentally sensitive and yet it would be correct to speak of heredity. This brings out one of Woodger's salient points, that it is useless to use ' heritable ' and ' acquired ' as mutually exclusive. Take haemophilia for example. In this context we are not concerned with the medical clinician's special use of terms ; from a broad biological point of view we must class this disease as hereditary. There is no space here to consider how we recognise members of the phenotype ; for simplicity let us deal only with those cases where we can recognise the disease before it has become manifest. As haemophilia is a convenient example, let us for convenience assume that we can test a person for it without cutting him. Now it might appear that the phenotype was environmentally insensitive, in that there is no environment in which the haemophiliac's blood will clot properly. But this is misleading. If a haemophilic adult has always lived in an environment in which he has never been cut, then he has never bled. Hence the phenotype is environmentally sensitive : it is sensitive to the difference between environments in which cuts occur and those in which they do not occur. This means that some environmental condition must be realised if the hereditary entity is to become manifest. To bring out the point it is useful to retain the concept of ' property ' in the non-capacity sense of quality, even if for no other reason, for the purpose of distinguishing it from ' capacity '. To make heredity clear we need some such concept as ' hereditary capacity '. (This has nothing to do with hereditary tendency.) But even this is not exactly what we require.

To take the next step consider haemophilia again. It is due to some failure in the clotting mechanism, which presumably occurs because of some structural defect. The failure of the blood to clot is due then to a deficiency, though not necessarily an absence, of some bodily substance. What we are concerned with, therefore, is not a capacity in general but what we may call a ' hypocapacity '. Likewise a hereditary immunity to certain diseases may be due to a ' hypercapacity '. We do not inherit the capacity to walk, though we may inherit the capacity to walk in a special way. What we inherit is not a capacity in general but the specific form of the capacity. Obviously two important types of specific capacity are hypocapacity and hypercapacity.

A further step has to be taken. We revert to the environmental condition that has to be realised in order that the hereditary specific capacity should be manifested. This requires, for haemophilia, the set of environments in which all adults receive a cut. Thus all of these environments, which must of course foster growth to adulthood, are required to be such as to bring about the manifestation of the specific capacity—they must have the appropriate ' inducing conditions ', such as knives or razor blades. Now the environments in question will not have these conditions all the time but only at certain times in the life of the people in it—our haemophiliac is not being perpetually cut. That is to say, the inducing conditions are potential but only sometimes realised. What we are concerned with, therefore, is environments with ' realised inducing conditions ', such as actual cuts. And thus the concept we need is that of ' environmental range of realised inducing conditions ', exemplified by the range of environments in which cuts occur. We are now in a position to define hereditary capacity :

> A specific capacity in members of a phenotype is hereditary if, and only if, the environmental range of realised inducing conditions is included in the environmental range associated with the phenotype.

It is worth adding a brief application of this idea. Heredity has come to have as an overtone of meaning what cannot be altered. This contamination with the idea of incurability has a grain of truth, for the most important things that are inherited, from the point of view of ordinary life and also of medicine, are hereditary incapacities. Obviously it is more difficult to make good a defect than to prevent a positive capacity from being manifested. It is easy to see, therefore, why such heredity complaints should lead to the idea that they are *ipso facto* incurable. On the other hand, a hereditary hypercapacity can usually be more readily prevented from materialising than a hypocapacity can be stimulated into action. Thus the capacity to grow tall could be interfered with by giving a suitable diet during the growing years. Much more difficult to alter than either the hypo- or hypercapacity, indeed well-nigh impossible to alter, are positive hereditary characters (not in the sense of capacities), such as the shape of the nose that runs in certain families. In short, hereditary characters are well-nigh impossible to alter, hereditary hypocapacities extremely difficult to alter, and hereditary hypercapacities relatively easy to alter. The socially or medically most important group is, of course, concerned with hereditary hypocapacities.

This treatment of the concept of heredity builds on Woodger's results ; but it has also been necessary to introduce further ideas not contained in his work, for it appears that he has produced material that would define only a part of the scope of heredity. This fact may be interpreted as a criticism of his symbolisation ; for, if it had the power and importance he claims for it, it should have led to the clarification of heredity. On the other hand, it may be interpreted as a tribute to his general procedure in promoting further development.

The space I have devoted to this topic may give a misleading impression that it is the author's sole concern. This is not so. I have selected it for discussion because it affords a suitable way of illustrating the content and method of the book and because of its special interest.

Even if Professor Woodger has not answered all the questions and even if his method may be questioned, his symbolisation should be carefully scrutinised and tried, for he has written a notable book.

J. O. Wisdom

Cerebral Mechanisms in Behaviour. The Hixon Symposium, edited by Lloyd A. Jeffress (John Wiley & Sons Inc., New York; Chapman and Hall Ltd., London, 1951. Pp. xvi + 311. $6.50; 52s.)

A Group of distinguished scientists drawn from many fields met at the California Institute of Technology in September 1948 to discuss the topic 'Cerebral Mechanisms in Behaviour'. The aim of the symposium was presumably to restore communication between psychologists, psychiatrists and neurophysiologists, but there were also important contributions from fields as diverse as mathematics, biology and chemistry. Several such symposia have been published in recent years. Some have been marred by the apparent absence of any editing, large sections being at a trivial conversational level. By contrast the present symposium has been well edited, and also the actual discussion must have been of a high order. It is therefore greatly to be regretted that the publication was so long delayed. There has been such a rapid advance that much has dated.

Professor von Neumann opened the symposium with an attempt to relate the principles of operation of calculating machines to the functioning of the cerebral cortex. There have now been several such attempts, but rarely has there been such a lucid exposition of the operational principles of calculating machines (automata as they are called) both of the analogy and digital type. However, though elements in the behaviour of the nervous system resemble operationally one or the other of these types, it does not appear that we have thereby any further insight into the functioning of the nervous system. It seems to the reviewer that the development of neurophysiology is likely to be impeded rather than aided by superficial analogies with automata. Despite all its grandiose claims cybernetics has contributed nothing to neurophysiology except the confusion of some neurophysiologists. The discussion following von Neumann's paper indicates that there was a failure of communication, except at a trivial level, between the mathematical and logical approach of von Neumann and the neurophysiological and psychological approaches of the other participants. No better illustration could be given than Professor Gerard's statement that he felt himself in 'the delightful but difficult rôle of hanging on to the tail of a kite'; a statement that provokes some delightful visual imagery! One further criticism concerns the section on the reproduction of automata. One may doubt if von Neumann expects us seriously to accept this logical game which is but a mere caricature of reproduction, for it involves the tacit assumption of a supervising genius who not only designs automata and has blue-prints of them, but also initially inserts instructions into them so that in principle they would go through the motions of a reproductive cycle!

Professor McCulloch gave a characteristically racy discourse under the arresting title: 'Why the mind is in the head'. Undoubtedly McCulloch is in agreement with the main stream of neurophysiological thought in that he attempts to make the generation and transmission of impulses the dominating and controlling features in cerebral mechanisms. But too often his imagination and rich verbal imagery run away with the scientific argument. One can sympathise with Dr Köhler's criticism that McCulloch introduces psychological terms into neurophysiology so that neurophysiological events will mistakenly be thought to have a psychological significance. The gulf between neurophysiology and psychology is speciously bridged. For example, a nerve impulse is called an ' atomic proposition on the move ', reverberatory activity in a closed chain of neurones is regarded as an ' eternal idea ', and neuronal circuits in the brain ' compute ' and may be graded in a scale of ' value '. It is unfortunate that McCulloch's illustrative diagrams were not included in the text. We feel ourselves at a great disadvantage in comparison with the discussion group, which gave a very fine account of themselves in the subsequent discussion, that on memory being particularly good. But unfortunately there was not enough criticism of the misleading and inaccurate statements in McCulloch's contribution. For example, it is stated that 10^8 photoreceptors in the eye each individually emit impulses, and that the threshold of the dark-adapted eye for light is about a photon in several seconds. However, McCulloch did succeed in creating the vivid impressionistic picture that stimulated the disputants to continue to make it the major topic of the brilliant general discussion a day later.

Dr Lashley provided the most thoughtful and scholarly contribution : ' The problem of serial order in behaviour '. He had many valuable suggestions to offer on the cerebral mechanisms involved in the expression of language and in other skilled movements. However, he rules out the control of such movements by sensory feed-back from muscle and joint on evidence which neurophysiologists would find unacceptable. Admittedly there is not time for the conscious cortical control of movement by such sensory feed-back, but the time for the proprioceptive control of movement at the spinal level and even at the cerebellar level is measured in hundredths of a second and hence would be operative in the fastest movements described by Lashley. In general one can say that psychologists tend to overlook or minimise the subconscious proprioceptive controls of movement, which were so effectively demonstrated by Sherrington. Nevertheless, the general thesis of Lashley gives the important concept of the dynamic operation of the spatio-temporal networks in the cerebral cortex, as distinct from the old view that he refers to as the ' static nervous system '. On the basis of this dynamic ever-active composite of interacting systems it seems possible to account for the wonderful properties of the cerebral cortex—the almost illimitable variety of performance both perceptual and conational and the great wealth of memory.

Under the title, 'Relational determination in perception', Professor Wolfgang Köhler gave a well-illustrated account of the electrical field theory of perception which stems from the Gestalt theory in psychology. It is postulated that the impulses discharged from a receptor organ such as the eye after relay to the appropriate cortical area give rise there to an electrical field of potential which bears some specific relationship to the form of the perceived object and which plays an intermediate rôle between the cortical nerve impulses and the actual perception. Superficially it might appear that a partial explanation was thus provided for the baffling transmution from a spatio-temporal pattern of impulses on the one hand to a percept on the other. However, there is just as great a problem in transmuting an electrical potential field to a percept. Furthermore, neurophysiologically this theory is, I think, becoming completely untenable, for there is no evidence that electrical fields generated in *normal* activity of the central nervous system play any rôle in modifying the activity of other neurones. All intercommunication appears to be synaptic, except perhaps in the synchronised activity of large populations of neurones such as may occur in convulsions. It should be stated that Professors Gerard and Weiss gave at least their qualified support to this electrical field theory of cortical action.

In more conventional contributions, Professors Klüver and Halstead gave experimental and clinical observations relating to the functions of specific parts of the cerebral cortex and in particular the structural correlates of intelligence. No special comment seems to be required for readers of this *Journal*.

It is to be regretted that there was no reference to the relationship of conscious mind to cerebral mechanism and to behaviour. It was apparently tacitly assumed that conscious mind could be ignored in its operational significance and that the problem of body-mind was now established beyond all dispute as a pseudo-problem, as for example is stated baldly by the neuro-anatomist, von Bonin, in his recent monograph, *An Essay on the Cerebral Cortex*. Yet many neurophysiologists are of the opposite opinion and reasoned arguments have been stated at length by Sherrington in *Man on his Nature*, and by neurologists such as Walshe and Russell Brain. It is, of course, not in doubt that all mechanisms of behaviour involve cortical activity, specifically the spatio-temporal patterns of activity in the networks of cortical neurones. But it is amazing and alarming to find that without discussion the whole of our conscious experience is being denied validity by a group of scientists discussing the behaviour of the cerebral cortex. They adopt as established the hypothesis of mechanical man without even realising what a tremendous act of faith is thereby involved, and how so much of experience is thereby arbitrarily jettisoned.

J. C. Eccles

Scientific Explanation : a study of the function of theory, probability and law in science, R. B. Braithwaite (The University Press, Cambridge, 1953. Pp. xii + 376. 40s.)

PROFESSOR BRAITHWAITE's book is based on the Tarner lectures of 1946. It is specially concerned with scientific deductive systems and their representations, the different levels of scientific laws and theories, and the use and misuse of models. A large part of the book deals with statistical hypotheses and the meaning of probability statements in a scientific system. Later chapters deal with laws of nature and the meaning of causality, teleological explanation, and explanation of scientific laws.

The author accepts the ' constant conjunction ' interpretation of scientific laws. Emphasis is laid on the fact that most scientific systems are complex, and that the refutation of an inference by a crucial test leaves a choice, since different parts of the system can be abandoned or, more usually, modified. Illustrations are given of the use of a formal calculus in a deductive system and of interpretations by models. There is an interesting discussion of the opinion that scientific concepts are logical constructs ; the chief conclusion is that such constructs could be made in many ways that cover the facts equally well, and consequently would be ambiguous. (My own view would be that constructs from sensations could not possibly be logical ones, without an extension of the meaning of logic ; errors of observation would prevent their coherence.)

A system based partly on the methods of Neyman and E. S. Pearson and partly on those of Wald is used to give a meaning to what I should call intrinsic probability (Carnap's p_2). Braithwaite agrees with me in finding the infinite series and infinite population definitions unsatisfactory, but hopes to make a definition in terms of the procedure used in trying to measure probabilities. The method is stated in terms of a procedure for rejection or non-rejection of a suggested value of a chance, but since rejection is not absolute a waste-paper basket capable of being searched to recover mistakenly rejected hypotheses is an essential part of the method. He differs from Mach (and agrees with me) with regard to ' economy of thought ' and in the need to draw a clear distinction between describing past and future experiences. He tries to give a justification of induction on ' previous successes' lines without using numerical assessments of epistemological probabilities ; he admits that the process is circular, but says that it is a type of circularity that does no harm.

The book is difficult to read, but that would apply to any adequate treatment ; but I do not think that it need be as difficult as it is. The method used for assessing probabilities would lead to appalling complications if it was applied to a continuous distribution, or, still worse, to a stochastic process. The rejection at each stage is in terms of an arbitrarily chosen number ; this arbitrariness is admitted but held to be unimportant in the limit. Still, there are methods that give answers for any finite number of observations.

In Braithwaite's admission that his justification of induction is circular he seems to be approaching the problem that I have called ' the revision of prior probabilities '.

His objection to the attempt to make a numerical theory of reasonable belief is that reasonable belief is an all-or-none reaction. My reply would be that an all-or-none reaction is never reasonable ; I might even quote Rivers, who gave this criterion precisely as the definition of an instinctive reaction. For scientific purposes we must have a graduated scale to express the acceptabilities of different suggested laws ; if anybody insists on interpreting ' reasonable belief ' to mean something else, he has the responsibility of suggesting a better name himself. Braithwaite frequently uses the general theory of relativity as an illustration, but in that very case it is doubtful whether two of the three crucial tests give results in accordance with the theory or not, and astronomers familiar with the problem are hesitant. It is a matter of considerable scientific importance whether the present discrepancy in the eclipse deflexion of light is 1, 2, 3 or 4 times the standard error ; one waste-paper basket, in Braithwaite's sense, would be quite inadequate.

The reasoning in the book is acute, and as the attitudes to scientific method discussed are more or less widely held it is worth while that the discussion should be thorough. I do not think anybody would be convinced by Braithwaite's justification of induction unless he was satisfied already ; for myself, being already satisfied, I prefer to break the circle, whether it is vicious or not, and try to frame a set of postulates that, so far as is possible, will combine the merits of the various incoherent ideas that pass for scientific method among scientists. As Braithwaite's method stands, it would ultimately give the same answers as mine but is unnecessarily clumsy.

HAROLD JEFFREYS

Miracles ; An Exercise in Logical Mapwork, Ian T. Ramsey (Clarendon Press, Oxford, 1952. Pp. 24. 2s. 6d.)

CAN scientific language ever be by itself perfectly adequate ? This is Professor Ramsey's opening question. No, he replies, because the very method of science is to pursue greater comprehensiveness at the cost of greater abstractness. The word ' miracle ' has no place in scientific language; not because science proves that ' miracles do not happen ', but because scientific description is elaborated with the precise aim of avoiding words like ' miracle '. In contrast with scientific language, historical language does not gain its extension by the scientific techniques of expansibility and convertibility ; it has quite a different logic. It ' is a technique for naming and organising *at a concrete level of personal encounter*, such a selection of facts as endeavours to repeat certain " events as they occurred ", and thus to bring them into relation with contemporary experience ' (p. 11). What, then, of metaphysical language? It supplements the scientific languages by answering their ' limiting questions ' and restoring concreteness to their abstractive patterns. Also, it provides historical language with the viewpoint for its selective activity and its extension. Metaphysical words (such as ' activity ', ' person ' and ' I ') both round off a subordinate language like a ' boundary ' and assist with factual reference like an ' index '. The important point is made that the ' index-language ' is, in Professor Ramsey's term, a ' Kata-language ', ' whose words can be used, and in various ways, in all the subordinate languages, though they belong to the logic of none '. Hence they lead to permanent puzzles.

Placing ' miracle ' on the language-map means placing ' God's activity ' on it. ' A miracle is an historical event which *a fortiori* demanding for its description " personal activity ", needs also the description " God " ' (p. 21). ' The nonconformity of miracles is thus to be related to their historical significance and extension and not at all to their scientific illegality, which is, indeed, a pseudo-category ' (p. 21). ' In asking the question " On such and such an occasion, did a miracle ' M ' occur ? " we are, if we ever give an affirmative answer, also commending theism as a particular Kata-language, a particular metaphysical index ' (p. 22). We are also, of course, passing a positive judgment on the evidential material.

Such, in summary form, is the theme of Professor Ramsey's inaugural lecture in the Nolloth chair at Oxford. ' We have tried ', he says, ' to set the word " miracle " on a total language map which includes a metaphysical index ; a map by which all discursive knowledge (and as examples we have taken science and history) is integrated, and whose unity, on the view I have barely outlined, relates to the activity of God himself ' (p. 23).

Professor Ramsey's discussion should interest others than theologians. In particular, his exposition of the way in which scientific theories continually increase in comprehensiveness and abstractness in order to preserve the applicability of the scientific method casts light upon a good deal that has been puzzling in the scientific development of the last fifty years. It is much to be hoped that his lecture will later be expanded into a full-length book.

E. L. MASCALL

Cosmology, H. Bondi (Cambridge Monographs on Physics, Cambridge University Press, 1952. Pp. 179. 22s. 6d.)

THIS book gives in a condensed form an account of the various theories of cosmology which may be regarded as of current as distinct from historical interest. It also attempts to estimate the scientific validity and importance of these theories. It may be said at once that the first part of this dual task is performed admirably. The reader who is familiar with the general character of the mathematical principles involved, though not necessarily expert in the minutiae of their application, is given just the sort of description he requires in order to understand the essence of the various theories without becoming lost in the details. In a purely scientific journal this aspect of the book would deserve further attention and praise, but our concern here is with the discussion of the theories in the light of scientific principles, and on this matter it is difficult in a brief review to convey the unsatisfactoriness, to use the mildest possible term, of the whole treatment. All that can be done is to select a few passages by way of illustration.

The fundamental postulate of cosmology is held to be the 'cosmological principle' ('the universe presents the same aspect from every point except for local irregularities', p. 11). The precise status of this principle, however, is somewhat elusive, since one can regard it as anything from a necessary requirement of 'the fundamental assumption of all physical science—the indefinite repeatability of experiments' (p. 11) to a hypothesis based on scanty empirical data ('the velocity-distance relation is the strongest evidence in favour of the cosmological principle', p. 40). Further, the phrase, 'except for local irregularities' leads one to expect that the principle is a statistical one, yet when another theory than the author's which ventures to employ it is under discussion we are told that 'the difficulties standing in the way of basing a theory on a statistical cosmological principle are very great indeed' (p. 137). Most of us will be unable to believe that, if it should be discovered that some parts of the universe differ from others, science will be destroyed at the roots, and we must therefore look at the empirical evidence for the principle. The 'velocity-distance' relation is the name given to the discovery that, according to some rather speculative estimates of the distances and velocities of the nearer extra-galactic nebulae, the distances are roughly proportional to the velocities. Owing to practical difficulties, the region surveyed is but a small fraction of the actual universe, and the cosmological principle is therefore a very great extrapolation to what is possibly infinite space. Why should we make it? Apparently in order to 'explain' the velocity-distance relation, for: 'the velocity-distance relation is the strongest evidence in favour of the cosmological principle. Any theory adopting the cosmological principle immediately arrives at the velocity-distance relation, whereas any theory attempting to do without it must make special *ad hoc* assumptions to explain it' (p. 40). Thus the *ad hoc* assumption of the cosmological principle is justified because if you reject it you must make *ad hoc* assumptions. The author, in fact, throughout shows an engaging tendency to call his own arbitrary assumptions, principles, and other people's legitimate generalisations, arbitrary assumptions. For instance, on page 131 we read: 'Milne then made the assumption, which is evidently suggested by ordinary physics, that matter is conserved. This is the second arbitrary though plausible assumption differentiating kinematic relativity from the steady-state theory', and on page 142 : 'General relativity makes the arbitrary assumption that its field equations apply in spite of changes in the universe'. On the other hand, the arbitrary assumption that the universe presents the same aspect, not only everywhere but at all times also, is called the 'perfect cosmological principle'. Incidentally, Mr Bondi seems quite unable to distinguish 'mass' from 'matter', and the two terms are bewilderingly interchanged in the same argument.

From the wealth of fallacies provided, two more instances must suffice. 'When the fundamental assumptions of physical science are investigated it is found that they are illogical and untenable unless certain properties are assigned to space and time' (p. 4). Kant might have been excused for thinking this, but how comes it to be asserted in the mid-twentieth century ? Finally, 'one of the weightiest objections brought against the hypothesis of the relativity of inertia is that it has not proved possible, so far, to put it into a mathematical theory' (p. 32). This idea that the 'truth' of a hypothesis depends on its mathematical potentialities reminds one of the story of the man who searched beneath a street lamp for a dropped coin. 'Are you sure you dropped it here ?' he was asked. 'No', he replied, 'I dropped it round the corner, but there is more light here.'

The fact that a really valuable account of cosmological theories can be so smothered by false philosophising is the strongest possible argument for the existence of the Philosophy of Science Group and of this *Journal.*

Herbert Dingle

The Impact of Science on Society, Bertrand Russell (George Allen & Unwin, Ltd., London, 1952. Pp. 140. 7s. 6d.)

The impact of Bertrand Russell on society, like that of science, has been disturbing and stimulating, and continuous during the lifetimes of most of us. He now works, he says, a mere four hours a day, but he is still producing a book a year. He is overtaking Plato (who dropped out of the race at eighty), drawing level with Bentham (who was still 'codifying like any dragon' at eighty-one), and lessening the lead of Hobbes (of whom Russell himself has admitted : 'I cannot discover that he wrote any large books after the age of eighty-seven.')

An ordinary octogenarian may be excused for repeating himself, while one who has written as much with such brilliance and enjoyable malice as Bertrand Russell has every justification for doing so. This book contains a good deal that is reminiscent of *The Scientific Outlook*, *Power*, and *Authority and the Individual*, while there is a long quotation from the essay on Pragmatism. Science is mainly considered *qua* technology; moreover, the book discusses the impact on society of many things somewhat loosely connected with technology, such as advertising, birth-control, large-scale organisation, and totalitarian control. Nightmarish possibilities and frightening probabilities are entertained. But as Russell admits of the typical recipient of such scarifying prophecies: 'You may make his flesh creep, but he rather enjoys the sensation.'

J. W. N. WATKINS

The Works of George Berkeley, Bishop of Cloyne, edited by A. A. Luce and T. E. Jessop, Vol. V, edited by T. E. Jessop (Thomas Nelson & Sons Ltd., London and Edinburgh, 1953. Pp. xii + 235. 30s.)

THIS volume contains all Berkeley's writings on tar-water, that is, letters and addenda, in addition to the famous *Siris*. Readers might be reminded that *Siris* contains only 135 pages. Since it is an attractive piece of intellectual romancing by a great thinker, it is worth reading, although neither the virtues of tar-water nor Berkeley's theory about it can be taken seriously.

Philosophers of science should have their attention drawn to the latter part of *Siris*, which contains a discussion of the philosophy of Newtonian physics, similar to that in *De Motu*, sometimes better expressed. Since the appearance of the Berkeley number of this *Journal* it is no longer necessary to underline the importance of this.

The same editorial care has been lavished on this volume as on the preceding ones. It is curious, however, that both editors, dealing respectively with *De Motu* in Vol. IV and *Siris* in Vol. V, should have avoided mentioning the importance of Berkeley's philosophy of physics.

J. O. WISDOM

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(*a*) Books Received for Review

Inclusion of books in this list does not preclude their being reviewed in later issues

V. E. Alperi, *Atomos Idea: l'origine del concetto dell 'atomo nel pensiers greco,* Felice le Monnier, Firenze, 1953, pp. viii + 214, Lire 1200.

The Works of Archimedes, edited in modern notation with introductory chapters by T. L. Heath with a supplement *The Method of Archimedes* recently discovered by Heiberg, Dover Publications, Inc., New York, new edition, 1953, pp. clxxxvi + 326 + 51, paper $ 1.95, cloth $ 4.95.

A. H. Basson and D. J. O'Connor, *Introduction to Symbolic Logic,* University Tutorial Press, London, pp. viii + 169, 7s. 6d.

J. D. Bernal, *Science and Industry in the Nineteenth Century,* Routledge & Kegan Paul, London, 1953, pp. xii + 230, 12s. 6d.

Ernst Cassirer, *Substance and Function* and *Einstein's Theory of Relativity,* unabridged edition, Dover Publications, Inc., New York, 1953, pp. xii + 465, paper $ 1.95, cloth $ 3.95.

Maurice Comforth, *Historical Materialism,* Lawrence & Wishart, London, 1953, pp. 206, 10s. 6d.

Maurice Daumas, *Les instruments scientifiques aux XVII^e et XVIII^e siecles,* Presses Universitaires de France, Paris, pp. iv + 417, no price stated.

J. L. E. Dreyer, *A History of Astronomy from Thales to Kepler,* Dover Publications, Inc., New York, new edition, pp. x + 438, paper $ 1.95, cloth $ 3.95.

Albert Einstein, *L'éther et la théorie de la relativité,* and *La géometrie et l'expérience,* traduit par Maurice Solovine, Gauthier-Villars, Paris, 1953, pp. 29, 300 fr.

Anthony Flew, *A New Approach to Psychical Research,* Watts & Co., London, 1953, pp. viii + 161, 10s. 6d.

E. W. Hobson, H. P. Hudson, A. N. Singh, and A. B. Kempe, *Squaring the Circle and Other Monographs,* Chelsea Publishing Company, 1953, pp. x + 57 + 144 + viii + 110 + vi + 51, $ 3.25.

Ernest Jones, *Sigmund Freud : Life and Work,* Vol. I, The Young Freud, The Hogarth Press, London, 1953, pp. xvi + 454, 27s. 6d.

Norman Kemp Smith, *New Studies in the Philosophy of Descartes : Descartes as a Pioneer,* Macmillan & Co., Ltd., London, 1952, pp. xii + 369, 25s.

Norman Kemp Smith, *Descartes' Philosophical Writings : Selected and Translated,* Macmillan & Co., Ltd., London, 1952, pp. ix + 317.

Irving F. Laucks, *A Speculation in Reality,* Philosophical Library, New York, 1953, pp. 153, $ 3.75.

S. F. Mason, *A History of the Sciences: Main Currents of Scientific Thought,* Routledge and Kegan Paul, Ltd., London, 1953, pp. viii + 520, 28s.

Jean Piaget, *Logic and Psychology,* with an Introduction by W. Mays, Manchester University Press, Manchester, 1953, pp. xx + 48, 6s.

Charles E. Raven, *Natural Religion and Christian Theology : Experience and Interpretation,* The University Press, Cambridge, 1953, pp. viii + 227, 21s.

Ian Rawlins, *Aesthetics and the Gestalt*, Thomas Nelson & Sons, Edinburgh & London, 1953, pp. xii + 227, 18s.

The Teaching of Philosophy: an international enquiry of Unesco, Unesco, Paris, 1953, pp. 230, obtainable from H.M. Stationery Office, 9s. 6d.

Håkan Törnebohm, *A Logical Analysis of the Theory of Relativity*, Almquist & Wiksell, Stockholm, 1952, pp. 273.

J. O. Wisdom, *The Unconscious Origin of Berkeley's Philosophy*, the Hogarth Press and the Institute of Psycho-Analysis, London, 1953, pp. xii + 244. 25s.

(*b*) JOURNALS

The contents of the latest issues of other journals are printed with the advertisements on the cover of this issue

Dialectica (Zürich, Switzerland), 1953, **7**, No. 2

- H. L. Mieville, ' Des constituants formels de l'idée de verité et de leurs signification ontologique '
- D. Christoff, ' Pensée formelle et pensée dialecticienne '
- S. Gagnebin, ' Brèves réflexions sur le principe d'identité et la norme de concordance (réponse à M. Mieville) '
- F. Gonseth, ' Réponse à M. H. L. Mieville '
- H. B. Curry, ' Theory and Experience '
- G. H. Müller, ' Andreas Speiser : Die mathematische Denkweise '

(*c*) ARTICLES

John Anderson, ' The Freudian Revolution ', *The Australian Journal of Philosophy*, 1953, **31**, 101

Max Born, ' The Conceptual Situation in Physics and the Prospects of its Future Development ', *Proceedings of the Physical Society, A*, 1953, **66**, 501

B. v. Freytag Löringhoff, ' Über die Bedeutung der Mathematik für die Philosophie ', *Studium Generale*, 1953, **10**, 600

T. A. Goudge, ' Organismic Concepts in Biology and Physics ', *The Review of Metaphysics*, 1953, **7**, 282

Adolf Grünbaum, ' Relativity, Causality and Weiss's Theory of Relations ', *The Review of Metaphysics*, 1953, **7**, 115

James C. Haden, ' The Challenge of the History of Science, Part II ', *The Review of Metaphysics*, 1953, **7**, 262

B. v. Juhos, ' Die neue Logik als Voraussetzung der wissenschaftlichen Erkenntnis ', *Studium Generale*, 1953, **10**, 593

S. Lilley, ' Cause and Effect in the History of Science ', *Centaurus*, 1953, **3**, 58

P. Lorenzen, ' Die Allgemeingultigkeit der logischen Regelen ', *Studium Generale*, 1953, **10**, 605

G. Martin, ' Methodische Probleme der Metaphysik der Zahl ', *Studium Generale*, 1953, **10**, 610

S. F. Mason, ' The Idea of Progress and Theories of Evolution in Science ', *Centaurus*, 1953, **3**, 90

A. Pannekoek, ' The Discovery of Neptune ', *Centaurus*, 1953, **3**, 126

Wesley C. Salmon, 'The Uniformity of Nature', *Philosophy and Phenomenological Research*, 1953, **14**, 39

Alfred Schuetz, 'Common-Sense and Scientific Interpretation of Human Action', *Philosophy and Phenomenological Research*, 1953, **14**, 1

W. Stegmuller, 'Bemerkungen zum Wahrscheinlichkeitsproblem', *Studium Generale*, 1953, **10**, 563

Patrick Suppes, 'A Set of Independent Axioms for Extensive Quantities', *Portugaliae Mathematica*, 1951, **10**, 163

J. B. Thornton, 'Scientific Entities (II)', *The Australasian Journal of Philosophy*, 1953, **31**, 73

Paul Weiss, 'Grünbaum's Relativity and Ontology', *The Review of Metaphysics*, 1953, **7**, 123

ANNOUNCEMENT

Centre for the Philosophy of Science

A GRANT by the Louis W. and Maud Hill Foundation of St. Paul has made possible the formation of the *Minnesota Center for Philosophy of Science*, which is administratively a department of the University of Minnesota in Minneapolis. The present research staff includes: Professor Herbert Feigl, Director; Professor Wilfrid Sellars (Chairman, Philosophy Department); Professor Paul Meehl (Chairman, Psychology Department); and Michael Scriven (M.A., Melbourne, Instructor in Philosophy).

Research on the logical and methodological structure of psychology will be undertaken this year; possible areas for future study include the foundations of probability and induction, and conceptual problems of physics, biology and the social sciences. There will be a number of small conferences to which scholars from other universities will be invited.

ERRATA AND CORRIGENDA

THE Editor regrets that, owing to postal delays with Europe during a railway strike, the following Errata and Corrigenda to Mr Kreisel's article 'A Variant to Hilbert's Theory of the Foundations of Arithmetic', which appeared in the August Number, arrived too late for the changes to be made in the article.

p. 108, line 10 *read* formalisations *for* formatisations.
p. 113, line 5 *read* $f(n)$ *for* (n)
p. 117, line 2 from bottom *read* $\underset{f}{\Lambda}$ *for* Λ.
p. 118, line 16 *read* $\rightarrow T[f(z)$ *for* $T[f(z)$
p. 119, line 18 *read* a_r *for* A_r.
line 20 *read* $\bullet b$ are *for* $\cdot b$ are.
line 28 *read* $a'_0 \bullet\!> a'_1 \bullet\!>$ *for* $a'_0 \cdot\!> a'_1 \cdot\!>$.
line 29 *read* 2^{a_1} *for* 2^{a_2} (twice).
line 30 *read* 2^{a_1} *for* 2^{a_2}.
p. 120, lines 7 and 8 *read* $a <\cdot\, b$ *for* $a < b$.
line 17 *read* symbol for functions *for* symbol.
lines 17 and 18, *delete* (and . . . ordering).
p. 124, line 2 from bottom *insert* 1951, **122,** *before* pp.
p. 126, line 14 *read* $\mu_m(n')(Em')(x)$ *for* $\mu_m(n')(Em')$.
line 17, *delete* (with analogous t).
line 30, *insert* (with analogous t) *after* $A(\mathrm{t}, x)$.
p. 127, line 21, *read* $A^*(b)$ *for* A^*b.
p. 128, line 7 *read* $\underset{f}{V}(x)(Ey)[A(f, x, y).$ *for* $\underset{f}{V}[(x)\ (Ey)A(f, x, y).$
p. 128, *delete* lines 24 and 25, *and replace by*

$$\{f(x_1) = 0 \rightarrow f[p(x_1,x_2)] = 0\} \,\&\, \{f(x_1) = 1 \rightarrow f[p(x_1,y)] = 1\} : \& : S(x_1) \,\&\, E(x_1) \bullet\!\rightarrow\!\bullet \{f(x_1) = 0 \rightarrow f[p(x_1,y)] = 0\} \,\&\, \{f(x_1) = 1 \rightarrow f[p(x_1,x_2)] = 1\}.$$

p. 128, after line 27 *add new paragraph* :

This result answers the question raised in line 3 on page 119, since $\underset{f}{V}(x)\ (Ey)\ A(f, x, y)$ is equivalent to $\underset{f}{V}\underset{g}{V}(x)\ A[f, x, g(x)]$.

p. 128, line 3 from bottom *read* $a <\cdot\, b$ *for* $a < b$ (twice).
p. 129, line 4 *read* $a <\cdot\, b$ *for* $a < b$.

In Professor Smart's 'A Note on Categories' in the previous Number, p. 227, line 22 *delete* while.

END OF VOLUME IV

ERRATA AND CORRIGENDA

[illegible]

p. [illegible], line [illegible] read [illegible]

p. [illegible], line 4 from bottom read [illegible]

p. [illegible], line [illegible] read [illegible]

p. [illegible], line [illegible] read [illegible]
line [illegible] read [illegible]
line [illegible] read [illegible]
line [illegible] read [illegible]
line [illegible] read [illegible]

p. [illegible], lines 7 and 8 read [illegible]
line [illegible] read [illegible]
line [illegible] read [illegible]

[illegible]

This error [illegible]

p. [illegible], line [illegible] from bottom read [illegible]

p. [illegible], line [illegible] read [illegible]

In Professor [illegible] "A Note on [illegible]" in the previous Number, p. [illegible], line [illegible] read [illegible]

END OF VOLUME [illegible]